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ART. XVII.—*Extract from a Discourse pronounced by* M. ELIE DE BEAUMONT *at the funeral of* M. ALEXANDRE BRONGNIART, *October* 9, 1847.*

M. BRONGNIART was born at Paris in 1770. His father was justly distinguished in the fine arts; and the intelligence of the son was developed in the midst of that brilliant society, which at the close of the last century his father had collected around him. He drew from the conversations of Franklin the germ of a mild and practical philosophy, from which he never departed; and from Lavoisier, the first notions of chemistry which was in part the foundation of his scientific career.

He early exhibited that *clearness* of elocution, which was one of his merits as a Professor, and it is related that Lavoisier himself was pleased to attend a lecture on chemistry given by the young Brongniart when but fifteen years old. He early finished his first scientific studies in the School of Mines of Paris, founded by Louis XVI, where M. Sage taught him mineralogy.

At twenty years of age, in 1790, he made a scientific tour. He visited England, where the mines and picturesque regions of Derbyshire made a vivid impression upon him, and where he collected the elements of a memoir upon the art of enameling, which was his debut, in the art of pottery and porcelain. His uncle, Demonstrator of Chemistry in the Garden of Plants, selected him as assistant and taught him practical chemistry. He studied also in the School of Medicine, where he took three "inscriptions:" and when the first requisition called all Frenchmen to

* Translated for this Journal from the "Institut," Oct. 13, 1847.

the frontier, he was attached to the army of the Pyrenees as Pharmaceutist.

The leisure afforded by a sojourn of fifteen months in these mountains, enabled him to study their rich and varied zoology and botany. He also made geological observations which afterwards proved valuable to science, and which it gratified him often to recall; but he encountered dangers which his youth never anticipated, and he was placed in prison on suspicion of having favored the escape of the learned naturalist, Broussonnet.

Restored to liberty after the 9th thermidor, (July 28,) he returned to Paris, where on the recommendation of Fourcroy and Coquebert de Montbret, who was then engaged in statistical mineralogy, he was attached to the agency of mines, in the capacity of engineer of mines. Soon after he was called to the Professorship of Natural History in the Central school of the Quatre-Nations; he became a contributor to the best scientific periodicals of the day, and a little after the 18th brumaire, (Nov. 9,) 1800, he was appointed director of the Porcelain Manufactory of Sèvres, on the recommendation of Berthollet.

Upon the organization of the Imperial University, Brongniart was appointed to prepare an elementary treatise on Mineralogy. This work, which appeared in 1807, was one of the best, especially the clearest and most practical, which had been published on this science, at that time so little accessible. There is an originality of explanation entirely peculiar, and a perspicuity worthy of being a model to authors who wish to bring science to the comprehension of youth. This book became the text of those finished lectures which M. Brongniart gave for a long time in the Faculty of Sciences as *associate* of M. Haüy and which he continued at the Museum of Natural History, when he was called to succeed this illustrious master. But M. Brongniart was not confined to mineralogy alone. His works were as various as his studies. He continued for a long time to cultivate zoology, and his early labors are not forgotten.

We owe to him the division of the reptiles into four orders, and all naturalists, with Cuvier at their head, have adopted from him the names *Saurians*, *Batrachians*, *Chelonians* and *Ophidians*; names which appear at this day so natural that they are constantly repeated without recalling their author. Subsequently he introduced the name *Trilobites*, and he laid the foundation of the classification of these singular Crustacea—different from all recent forms—in a learned memoir which has been the starting point of all the works on this large family.

Independently of the honor it conferred, M. Brongniart owes perhaps to his zoological labors one of the greatest pleasures of his life, viz., his intimate relation with the illustrious author of the "Règne Animal" and "l'Anatomie Comparée."

When Cuvier was called to Paris, M. Brongniart perceived at once the lofty character of his genius. Justly appreciating his vast intelligence, he became one of his most faithful friends and sincerest admirers: and this noble sentiment was cherished to the end of life. But it was not confined merely to admiration: it led him in his appropriate department to further one of the greatest labors of his friend.

The masters of science have proclaimed the discoveries of M. Cuvier in the fossil bones of the plaster beds of Montmartre, the most original of all those which have contributed to his high and lasting reputation. It was impossible to derive from these discoveries their principal consequences before the strata in the environs of Paris had been examined mineralogically and classed geologically in their order of superposition. The eminently discriminating judgment of Cuvier perceived the necessity of assistance in prosecuting those researches to complete success, the object of which was peculiarly if not exclusively his own.

In 1808 M. Brongniart traveled into Auvergne,* where he ascertained the existence of certain freshwater formations containing only fossil fluviatile shells. This was a new application of zoology to the study of mineral strata. Cuvier then clearly perceived in him, the collaborator whom nature had designated, not as another Daubenton, (a glorious example,) but a spirit perfectly congenial and possessed of an originality wonderfully adapted to his own. Being already interested in such subjects, through his previous studies with regard to the gypsum of Montmartre and the limestone of Champigny—himself the leader of this school of geologists, and seconded in certain details by his youthful pupils, then and since become celebrated Professors,—by M. Beudant, by Constant Prevost, by M. Desmarest, Junr.—M. Brongniart presented to the first class of the Institute, in the month of April, 1810, in concert with M. Cuvier, *The Essai upon the mineral geography of the environs of Paris*, which first appeared in the Journal of Mines and which soon became so celebrated. Being republished in 1811 enlarged and improved, this work, in which are laid down the first rules of the application of zoology to the characterizing of geological formations, and which still remains the classical model for works of this nature—this work, in 1815, opened to M. Brongniart the doors of the Academy of Sciences. In that institution he succeeded the indefatigable explorer of the volcanos of Auvergne, Desmarest, whom he surpassed in renown.

M. Brongniart never relaxed his exertions. His admission into the Academy redoubled his activity. With an enthusiasm equally cautious and fruitful of the best results, pressing forward

* The celebrated region of the extinct volcanos of Central France.—Eds.

into the vast domain which his sagacity had surveyed beforehand, he became, in a few years, the lawgiver of this part of geology, then as novel as it is important, established its law on well founded facts, and marching forward in his career, every step proved an important discovery. As a conqueror of a new order, M. Brongniart set forth in 1817 for Switzerland, the Alps and Italy, attended by his son and by M. Bertrand Geslin, one of his most accomplished pupils. In these countries, where Saussure had been already immortalized, he erected, in a manner the most surprising, those landmarks which still stand and which now are the occasion of discussions only as to details, and without which, no department of natural history can be perfect. Who has not been struck with the boldness, as happy as it is astonishing, which associated the black limestones of the mountain of Fis, in Savoy, with our cretaceous formations in Northern France! In 1822, all the results of this nature that had been obtained were grouped in a second (third?) edition of *The Geological Description of the environs of Paris*, which thus became emphatically the monument of Brongniart's genius. But he did not stop with the modern formations, which were the particular object of this great work. Subsequently, with the same object in view, M. Brongniart visited Norway and Sweden. He was there welcomed by Berzelius, who volunteered to act as his guide and interpreter in a country with whose language he was unacquainted.

On that occasion, M. Brongniart laid the foundations of the classification of the most ancient fossiliferous formations. It was also during this journey in Scandinavia that M. Brongniart collected the materials of a memoir upon *erratic blocks*, which has so happily associated his name with those of Saussure and von Buch, in the investigation of a phenomenon by which the grandeur of the revolutions of the globe is recorded in the most striking manner.

I must not omit to mention his memoir—so remarkable for originality—upon the ophiolites (serpentines) of the Appennines; nor his observations, so exact and ingenious, upon volcanos and especially upon Vesuvius. But you cannot expect, gentlemen, that in this brief recapitulation I should recount all the results with which the labors of sixty years have enriched the sciences.

M. Brongniart did not confine himself to a single branch; nor did he content himself with theoretical speculations. The cares, labors and researches, which occupied him during forty-seven years, while he was director of the Royal Manufactory of Porcelain of Sèvres, would have honorably filled the life of an ordinary savant. Being occupied with numerous journeys, the object of which was to make him acquainted with all the grand manufactures of the same kind existing in Europe, and with all the

resources for their primary materials, he derived from these sources the elements of a beautiful memoir, both geological and chemical, upon the kaolins (porcelain clays) which has been published in more recent years. Faithful to his first researches upon the art of enameling, he caused to be revived at Sèvres the nearly lost art of painting upon glass, the beautiful productions of which we have all admired.*

M. Brongniart, at the age of nineteen, was one of the founders of the Philomathic Society—a society equally scientific and modest, having for its motto, *Science and Friendship;* which has proved to be the centre of so many useful communications, and which at the epoch of the suppression of all the higher institutions, was able to hold on its course always illumined by the sacred fire of science.

ART. XVIII.—*On the Production of Light by Chemical Action;* by JOHN WILLIAM DRAPER, M.D., Professor of Chemistry in the University of New York. (Communicated by the author.)

THE production of light and heat by the combustion of various bodies, is of all chemical processes, that which ministers most to the comfort and well-being of man. By it the rigor of winter is moderated, and night made almost as available for our use as the day.

One would suppose that of a phenomenon, on which so much of our personal and social happiness depends, and which must have been daily witnessed by every man that has ever lived, all the particulars ought to have been long ago known. Among scientific men its importance has been universally recognized; the early theories of chemistry, such as those of Stahl and Lavoisier, are essentially theories of combustion.

It is nevertheless remarkable how little positive knowledge we still possess on this subject. Some chemists believe that the light emitted by flames is due to electric discharges; others, regarding light and heat as material bodies which can be incorporated or united with ponderable substances, suppose that they are disengaged as chemical changes go on. In this confusion of opinions a multitude of interesting and hitherto unanswered questions present themselves. It is known that different substances, when burning, emit lights of different colors; thus sulphur and carbonic oxyd burn blue, wax yellow, and cyanogen lilac. What are the

* M. Brongniart in 1844, published his splendid work on Pottery or "The ceramic arts," entitled, *Traité des Arts Ceramiques, ou des Potteries considerées dans leur histoire, leur pratique et leur theorie.* It is in two vols. 8vo, with an Atlas of 60 plates. We have alluded to his labors in this department, on page 142 of this volume.—EDS.

chemical conditions that determine these singular differences? How is it that by changing the circumstances of combustion we can vary the nature of the light? We turn aside the flame of a candle by means of a blowpipe, and a neat blue cone appears. Why does it shine with a blue light?

Such enquiries might be multiplied without end: but a little consideration shows, that their various answers depend on the determination of a much more general problem; viz.—*Can any connection be traced between the chemical conditions under which a body burns, and the nature of the light it emits?* It is to the discussion of that problem that this memoir is devoted.

Sir H. Davy has already furnished us with two important circumstances in relation to the nature of flame; 1st, all common flames are incandescent shells, the interior of which is dark; 2nd, the relative quantity of light emitted depends on the temporary disengagement of solid particles.

It is only by a very general examination of the light arising from various solids, vapors, and gases, when burning, that we can expect to obtain data for a true theory of combustion. This is what I shall endeavor to furnish on the present occasion.

As was foreseen by all the older chemists, the true theory of combustion, whatever it may prove to be, must necessarily be one of the fundamental theories of chemistry. It must include the nature of all chemical changes whatsoever. The subject is therefore not alone interesting in a popular sense, but of great importance in its scientific connexions.

I. *Prismatic analysis of the flames of various vapors and gases, proving that they yield all the colors of the spectrum.*

I commenced this investigation of the nature of flame, and of combustion generally, by an optical examination of various bodies in the act of burning. Some authors have asserted that certain flames yield monochromatic lights. It is necessary to verify this assertion if true, or set it aside if false.

The instrumental arrangement which I have employed is as follows: the rays of the flame of which the examination is to be made, pass through a horizontal slit $\frac{1}{30}$ inch wide and one inch long in a metallic screen, and are received at a distance of six or eight feet on a flint glass prism, the axis of which is parallel to the slit. After passing the prism they enter a small telescope, which has a divided micrometer, and also parallel wires in its eye-piece. Through this telescope the resulting spectrum is viewed.

If it be the flame of a lamp of any kind that is to be examined, by using a moveable stand, we are able to raise or lower it, and thus analyze different *horizontal elements*, in its lower, its middle, or its upper part at pleasure. If, instead of a horizontal, we wish to examine a *vertical element* of the flame, the slit and

the prism must of course be set vertically. The former mode possesses great advantages, as will be presently pointed out. It is to be understood, in all cases, that the eye-piece of the telescope is adjusted to give a sharp image of the slit, and the prism is at its angle of minimum deviation.

By this arrangement I have examined a great number of different flames, as those of oil, alcohol, solutions of boracic acid and nitrate of strontian in alcohol, phosphorus, sulphur, carbonic oxyd, hydrogen, cyanogen, arseniuretted hydrogen, &c. Among these it will be noticed different colors occur; oil gives a yellow flame, alcohol a pale blue, boracic acid green, strontian red, phosphorus yellowish white, sulphur and carbonic oxyd blue, hydrogen pale yellow, cyanogen lilac, arseniuretted hydrogen white, &c.

Notwithstanding this diversity of color, all these flames, as well as many others I have tried, yielded the same result. *Every prismatic color is found in them.* Even in those cases where the flame is very faint, as in alcohol and hydrogen gas, not only may red, yellow, green, blue, and violet light be traced, but even bright Fraunhoferian lines of different colors.

This observation holds good for those flames reputed to be monochromatic; for example, alcohol burnt from a wick imbued with common salt. It is not alone a yellow light which is evolved, the other colors plainly though more faintly appear.

All flames, no matter what their primitive colors may be, evolve all the prismatic rays. Their special tints arise from the preponderance of one class of rays over another; thus in cyanogen the reds predominate, and in sulphur the blues.

The production of light in the case of flames is thus proved to be a very complex phenomenon. The chemical conditions under which their burning takes place, are likewise very complex. The combustible vapor is surrounded on all sides by atmospheric air, diffusion occurs, and rapid currents are established by the high temperature. Such circumstances complicate the result, and it is only by observing the burning of an elementary solid, in which most of these disturbances are cut off, that we can hope to effect a proper resolution of the problem.

II. *Prismatic analysis of the light of an elementary solid burning at different temperatures; proving, that as the temperature rises, the more refrangible rays appear.*

I took from the fire a mass of anthracite coal, the fuel ordinarily used in domestic economy in New York, and which from its compactness, the intense heat it evolves, and other properties, appears to be well fitted for such investigations as the present. This coal was placed on a support so as to present a plane surface to the metal screen. The rays coming from it and passing the slit were received on a flint glass prism, and viewed through the telescope.

When the coal was first taken from the fire, and was burning very intensely, on looking through the telescope I saw all the colored rays of the spectrum in their proper order. I had previously passed through the slit a beam of the sun light, reflected from a mirror, that I might have a standard spectrum with fixed lines. Now when the coal was burning at its utmost vigor, the spectrum it gave did not seem to me to differ either as respects length or the distribution of its colors from the spectrum of sunlight. But as the combustion declined, and the coal burnt less brightly, I saw that its spectrum was becoming less and less, the shortening taking place at the more refrangible extremity, one ray after another disappearing in due succession. First the violet became extinct, then the indigo, then the blue, then the green; until at last the red with an ash gray light occupying the place of the yellow, was alone visible, and presently this also went out.

From numerous experiments of this sort, I conclude that *there is a connexion between the refrangibility of the light which a burning body yields, and the intensity of the chemical action going on; and that the refrangibility always increases as the chemical action increases.* It may perhaps be objected by some that in the form of experiment here introduced two totally different things are confounded; and that the burning coal not only gives forth its rays as a combustible body, strictly speaking, but also as an incandescent mass.

To avoid this objection as far as possible, and also to reach a much higher temperature than could have been otherwise obtained, I threw a stream of oxygen gas on that portion of the anthracite which was opposite the slit; but my expectations were disappointed, for instead of the combustion being increased, the coal was actually extinguished by the jet playing on it. I therefore replaced the anthracite with a flat piece of well burnt charcoal, kindled at the part opposite the slit, and on throwing a stream of oxygen on this part, the combustion was greatly increased, and through the telescope I saw a spectrum rivalling that of the sunbeams in brilliancy, all the colors from the extreme red to the extreme violet being present.

Now on shutting off the supply of oxygen the combustion of course declined, and whilst this was going on I looked through the telescope and saw the violet, the indigo, the blue, the green fade away in succession. By merely turning the stopcock, through which the oxygen came, I could reëstablish the original colors or witness their decline. And it was very interesting to see with what unerring regularity as the chemical action became more intense the more refrangible colors were developed; and how as it declined they disappeared in due succession. The final tint

being red, and that ash gray in the position of the yellow, which I have described in my former memoir.*

In the form of experiment here made, the combustion is of course merely superficial, and the rays come from the charcoal not as an incandescent but as a burning body.

III. *Of the constitution of flames; proving that they consist of a series of concentric and differently colored shells.*

I regard the foregoing experiments as affording the means of explanation of the much more complicated phenomena of flames; and proceed to inquire whether the principle I have just brought forward, of the coördinate increase of refrangibility and chemical action, will hold good for them: premising the experiments now to be detailed with the following considerations.

All common flames, as is well known, consist of a thin shell of ignited matter, the interior being dark; the combustion taking effect on those points only which are in contact with the air. From the circumstances under which the air is usually supplied, this ignited shell cannot be a mere mathematical superficies, but must have a sensible thickness. If we imagine it to consist of a series of strata, it is obvious that the phenomena of combustion are different for each. The outer stratum is in absolute contact with the air, and there the combustion is most perfect. But by reason of the rapid diffusion of gases into one another, currents, and other such causes, the atmospheric air must necessarily pervade the burning shell to a certain depth, and in the successive strata as we advance inward, the activity of the burning must decline. On the exterior stratum oxygen is in excess, at the interior the combustible vapor; and between these limits there must be an admixture of the two, which differs at different depths. Admitting the result of the foregoing experiments with anthracite coal and charcoal to be true; viz., that as combustion is more active, rays of a higher degree of refrangibility are evolved, it follows that *each point of the superficies of every flame, no matter what the combustible may be, must yield all the colors of the spectrum*, the violet coming from the outer strata, the yellow from the intermediate, and the red from those within. If we could isolate an elementary horizontal section of a flame, it should exhibit the appearance of a rainbow ring; and when those compound rays are received on the face of a prism, the constituent colors are parted out by reason of their different refrangibility, and the eye thus made sensible of their actual existence.

When thus, by the aid of a prism, we analyze the light that comes from any portion of the superficies of a flame, we in effect dissect out in a convenient manner and arrange together side by side, rays that have come from the different strata of the burning

* See this Journal, November, 1847, p. 392.

shell. These, without the prism, would all have pursued the same normal path, and produced a commixed effect on the eye; but with it are separated transversely, and each becomes perceptible.

It might be supposed that, in the familiar instance of an oil lamp, if we put any check on the supply of air, and thereby check the intensity of combustion, we ought to have the flame emitting rays of light the refrangibility of which becomes less and less, and which, from being quite white, should pass through various shades of orange, and end with a dull red. But the compound nature of the burning vapor interferes with that result. For, when a certain point is gained, the hydrogen for the most part alone burns, the carbon being set free as smoke, and such a flame cannot support itself in strict accordance with the principle given.

We must then search for other conditions under which carbon is found, and which are free from this difficulty. Two at once present themselves; they are carbonic oxyd and cyanogen gas. In the former the carbon is already united with half the quantity of oxygen required for maximum oxydation, its complete combustion can therefore be carried on with a limited supply of air. In the latter the carbon is united with nitrogen, which, during combustion, is set free, and interferes with the process by cutting off the more complete access of the atmosphere.

In place of the burning coal of the former experiments, I substituted a jet pipe through which the various gases might be made to pass, and the rays emitted by their flames enter the telescope after passing through the slit and prism. In this arrangement the slit should be horizontal, and not vertical. So far from being immaterial which of the two positions is selected, very great advantages arise from the former. If the slit be vertical, the prism, it is true, will separate the constituent colors from one another, but it fails to show their relative position. If it be horizontal, the relative positions of the different colors can be demonstrated; and it can be proved that a horizontal section of a flame is in reality, as has been already remarked, a colored ring, the red being the innermost color, and the violet outside. For if this is the order in which the colors occur, the red ring must necessarily have a less diameter than the green, and the green than the violet; and when the prism, set also in a horizontal position, separates these colors from each other, the sides of the resulting spectrum ought not to be parallel but inclined to each other, the breadth being least in the red, and increasing as we pass to the violet end. This increasing breadth proves that the constituent colored shells of the flame envelop each other, the violet being outermost and therefore broadest. This valuable indication would be wholly lost if the slit was vertical.

This being understood, I may illustrate the facts now to be brought forward by an example of the prismatic analysis of a horizontal element of the flame of a spirit-lamp—it being understood that the prism is at its angle of minimum deviation, and the spectrum seen through the telescope. All the prismatic colors in their proper order are visible, the sides of the spectrum not being parallel, the inclination being quite rapid toward the red extremity, the rays of which come from the interior of the flame where the diameter is less. Mere inspection is sufficient to show the rapid approach of the red sides to each other; and I satisfied myself that, even in the more refrangible regions, there is the same want of parallelism, by rotating the telescope on its vertical axis, so that the vertical wires in its eye-piece might coincide with first one and then the other side of the spectrum. It will be understood that I took the proper precautions not to be deceived by a partial want of achromaticity in the telescope, which might have led to a mistake.

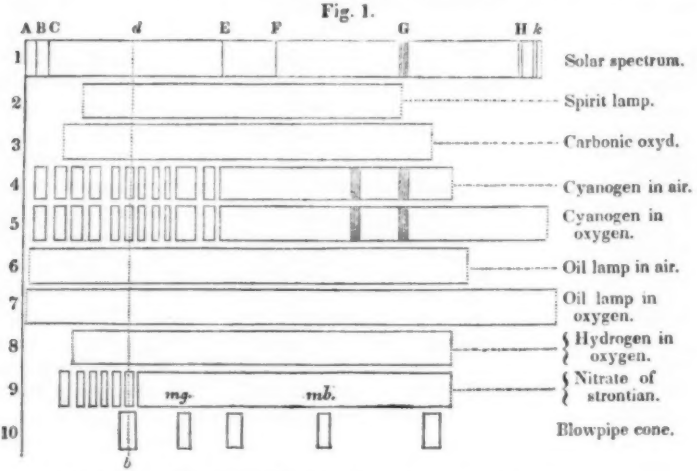
But further, the yellow space of such a spirit-flame spectrum is crossed by a bright fixed line, Sir David Brewster's monochromatic ray. It is a beautiful example of the principles just pointed out in this method of horizontal analysis, being of much greater width than the rest of the spectrum, and recalling to the imagination the appearance of Saturn's ring when nearly closed, and seen through a telescope of moderate power. This ray, from its superior breadth, must necessarily come from that pale tawny light which invests the bright part of the flame. This, which is readily seen when the flame is large, envelops the middle and upper parts, but cannot so easily be detected low down. It is to be attributed to the carbonic acid and steam that have risen at a high temperature in the burning shell, and are escaping at a degree above that of incandescence into the air, and are mingled with oxygen diffusing from the air into them. A similar tawny cloak surrounds the upper part of the flame of a candle; it answers to the oxydizing flame of the blowpipe, and yields Brewster's monochromatic yellow light.

IV. *Explanation of the nature of colored flames; shewing for example, why carbonic oxyd burns blue, and cyanogen gas red.*

To return now to carbonic oxyd and cyanogen: fig. 1, No. 1, represents the solar spectrum with its fixed lines; No. 3 represents the spectrum of carbonic oxyd burning in the air. It begins in the red region, short of the fixed line C, and terminates between the lines G and H. It yields therefore rays of every color, and this is in accordance with the principles I have laid down. But when the relative quantity and force of the rays is estimated, in comparison with the sun light spectrum, the red and orange are deficient, and the more refrangible colors predominate, and indeed it is the excess of these that gives the flame its characteristic blue

tint. This agrees with what has been observed as to anthracite and charcoal, for with carbonic oxyd a very limited supply of oxygen can bring about the maximum chemical action, and therefore liberate in abundance rays of maximum refrangibility.

Fig. 1.



Spectra of various flames.—The dotted line *d b*, is Brewster's yellow ray.

This condition of things is inverted in the case of cyanogen gas. It is the nature of its flame to be enveloped as it were in a sheet of nitrogen arising from its own burning; and this necessarily impedes the access of air, and checks the intensity of the chemical change; a check which is at once betokened by the emission of a predominant number of rays of low refrangibility, or of a red color.

But there is a striking difference in the conditions under which carbonic oxyd and cyanogen burn. In the case of the former the whole gas is combustible, in the latter the carbon alone, and we have in reality introduced an incombustible element into the flame; for as the carbon burns, the incombustible nitrogen is set free. It occurred to me, in selecting this gas for experiment, that this condition should impress a physical characteristic on the flame. I thought it was not impossible that dark lines in its spectrum might be the result; because there must be a peculiar arrangement of the burning strata, which together make up the shell of the flame, every two atoms of carbon setting free one of nitrogen. I did not know until subsequently that this flame had been examined by Mr. Faraday. Having therefore confined some cyanogen, made from the cyanid of mercury, in a glass gas-holder, which was filled with a saturated solution of common salt, I burnt it from the jet pipe, and found that what I had surmised

was actually the fact. There was a spectrum so beautiful that it is impossible to describe it by words or depict it in colors. It was crossed throughout its extent by black lines, separating it into well marked divisions. I could plainly count four great red rays of definite refrangibility, followed by one orange, one yellow, and seven green; whilst in the more refrangible spaces, were two extensive groups of black lines, recalling somewhat from their position, but greatly exceeding in extent, Fraunhofer's lines marked G and H in the sun's rays. I shall return to the consideration of this spectrum and to the nature of fixed lines presently, here only making the remark, that the burning of cyanogen, both as respects the color of the light, and the occurrence of fixed lines, is a direct consequence of the principle I am establishing.

The unassisted eye detects two well marked regions in the cyanogen flame; a greenish gray stratum on the outside, and a lilac colored nucleus within. Decomposed by the prism, a horizontal element of this flame shews that the exterior shell contains all the prismatic colors, except perhaps the yellow; but the green, the blue and the violet greatly predominate. The interior lilac flame is the source of the bright spectrum with fixed lines just described.

V. *Continuation of the same principle in the case where combustion is carried on in oxygen gas, instead of atmospheric air.*

If the principle that high refrangibility is connected with intense chemical action be true, it must hold good when the nature of the atmosphere in which the burning is carried forward is changed. If instead of being common air it is oxygen gas, we ought to be able to foresee the result. Carbonic oxyd, when made to burn in that gas, should not change its tint; because if the air can carry on the process to its maximum effect, oxygen can do no more. But the result should be just the reverse with cyanogen, which, if made to burn in oxygen, should be capable of emitting rays of a higher refrangibility.

Foreseeing this, I proceeded to submit the two gases to the test of experiment; and first arranged the carbonic oxyd that its spectrum might be examined in the telescope, as already described. Then, causing a clean bell jar full of oxygen to be inverted over it, the flame diminished somewhat in size, emitted a slight crackling sound, *but retained its color unchanged.* Its spectrum appeared precisely the same, both as respects extent, and the distribution of color, whether the burning took place in oxygen gas, or in atmospheric air.

If cyanogen be made to burn in oxygen, we should expect that it would lose to a great extent its characteristic lilac tint, and emit a whiter light. It was therefore very interesting to find, that the moment the flame was immersed in oxygen, it lost much of its pinkish color, and became of a dazzling brilliancy. And

on examination through the telescope, though all the colors had increased in brightness, the most remarkable effect took place among the extreme refrangible rays. Far out of the limits of the ordinary spectrum, a ray of great purity and force was developed, as represented in No. 5. Its color is violet.

I have made similar experiments on many other flames beside those here mentioned. It is not necessary to relate them in detail, for they give the same results. In every instance of combustion in the air, when the flame is bright enough, all the colors are visible; and when the combustion takes place in oxygen, they are increased in intensity. With hydrogen gas and alcohol, the light is so feeble that the eye cannot catch the terminal rays, but as soon as the combustion is made in oxygen, the red and the violet both appear, the latter however predominating. Several of these spectra both in air and oxygen are represented in fig. 1. In No. 9 the letters *m g* and *m b* indicate a maximum of green and of blue light in the form of bright lines.

It does not require the use of a prism to satisfy one's self of the change of tint that flames exhibit when the chemical action increases. In reality it is only necessary to contrast the color of the light emitted in air and in oxygen gas. In the latter case, rays of a higher refrangibility uniformly arise.

On the evidence furnished by the foregoing experiments, I regard all flames as consisting of a shell of ignited matter, in which combustion is going on with different degrees of rapidity at different depths; being most rapid at the exterior, where there is a more perfect contact with the atmosphere, and diminishing inwards. In a horizontal section, the interior space, consisting of unburnt vapor, is black; this is surrounded by a ring where the combustion is incipient, and from which red light issues; then follow orange, yellow, green, blue, indigo and violet circles in succession; the production of each of these tints being dependent on the rapidity with which chemical action is going forward, that is, on the amount of oxygen present, the tints gradually shading off into one another, and forming as I have already said a circular rainbow. An eye placed on the exterior of such a flame sees all the colors conjointly, and from their general mixture arises the predominant tint.

An examination of the flame of a candle *vertically*, confirms this conclusion. For the red projects on the top of the flame, and the blue towards its bottom.

From this, which may be regarded as the normal flame, the flame of cyanogen differs. It must consist of as many concentric strata as the prism separates it into regions of definite refrangibility. The interior part is therefore divided into four red layers, followed by one of orange, one of yellow, seven of green, &c. There are two great inactive spaces towards the outside of

the flame, corresponding to the two great groups of fixed lines. Perhaps through all these inactive parts, the incombustible nitrogen chiefly escapes.

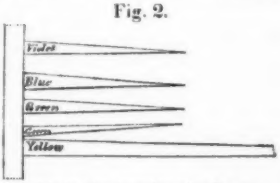
VI. *Effects of the introduction of air into the interior of a flame, producing the destruction of the red and orange strata, and converting them into violet.*

It now becomes a curious subject of enquiry to determine what must take place when an ordinary flame is disturbed by the introduction of air into its interior. When a blowpipe jet is thrown through the flame of an oil lamp, the sharp blue cone which forms, indicates, on the principles here set forth, that the combustion is much more active. But if the colors of a common flame come from different depths, the red being the innermost, it is clear that the introduction of a jet of air by a blowpipe should make the combustion rapid where before it was slowest, and the less refrangible colors ought to be destroyed. A prismatic analysis should exhibit the spectrum of a blowpipe flame without any red or orange.

In this examination no slit is required, as in the former experiments, for the cone itself when at a distance of six or eight feet, is narrow enough for the purpose. It yields a very extraordinary spectrum. As was anticipated, I found that all the red rays were gone, and not a vestige of either them or the orange could be seen. But the spectrum was divided into five well marked regions, separated from one another by inactive spaces. In short I saw five distinct images of the blue cone; one yellow, two green, one blue, and one violet. In fig. 1, No. 10, this result is represented.

This experiment may be verified without a telescope. On looking through a prism set horizontally, at its angle of minimum deviation, at the blowpipe cone some six or eight feet distant, there will be seen a spectrum of that part of the flame which does not join in the production of the blue cone. It contains of course all the prismatic colors. But projecting from this, are five separate colored images of the cone; one yellow, two green, one blue and one violet. They are entirely distinct from one another, and parted by dark spaces as seen in fig. 2.

Fig. 2.



Air in the interior of a flame.

Such is the effect of introducing air into the interior of a flame, and destroying those strata that yield the red and orange colors. The effect of a blowpipe is to produce a double stratum of blue light, one being external and the other internal; also two strata of green, one again external and the other internal; and the escaping products of combustion, steam and carbonic acid, mingled

with atmospheric air, constitute the oxydizing flame which envelops the blue cone, and emits Brewster's monochromatic yellow light. That the yellow light comes from this flame is proved by the greater length of its image.

VII. *Physical cause of the production of light by chemical action.*

Do not the various facts here brought forward, prove that all chemical combinations are attended by a rapid vibratory motion of the parts of the combining bodies, which vibrations become more frequent as the chemical action is more intense?

The burning particles which constitute the inner shell of a flame, are executing about four hundred billions of vibrations in one second; those in the middle about six hundred billions; and those on the exterior, in contact with the air, about eight hundred billions in the same time. The quality of the emitted light, as respects its color, depending on the frequency with which these vibrations are accomplished, increases in refrangibility as the violence of the chemical action becomes greater.

The parts of all material bodies are in a state of incessant vibration. That which we call *temperature*, depends on the frequency and amplitude of those vibrations conjointly. If by any process, as by chemical agencies, we increase that frequency to between four and eight hundred billions of vibrations in one second, ignition or combustion result. In the case of the former of these numbers, the temperature is 977° F. At this temperature or epoch, the waves propagated in the ether impress the organ of vision with a red light. *This also is the temperature of the innermost shell of a flame.* If the frequency of vibration still increases, the temperature correspondingly rises, and the light successively becomes orange, yellow, green, blue, &c., and this condition obtains in the successive strata of a flame, as we pass from its interior to its exterior superficies.

The general principle at which I thus arrive, as the final result of this experimental investigation, viz., that there is a connexion between the vehemence with which chemical affinity is satisfied and the refrangibility of the resulting light, assumes the position of a simple consequence of the undulatory theory. Is it not very natural, if all chemical changes are attended by vibratory motions in the particles of the bodies engaged, that those vibrations should increase in frequency or rapidity as the action becomes more violent? But an increased frequency of vibration is the same thing as an increased refrangibility.

I think that, in this manner, the theory of ethereal undulations is on the point of including many of those fundamental facts in chemistry, which until now have been believed to be adverse to it, or at all events as standing apart from it. I recall the admirable remark that Mr. Whewell has made in his history of the In-

ductive Sciences;—how this theory, like that of universal gravitation, has exhibited all the aspect of a great physical fact, advancing to the explanation of things that seemed to have no necessary connexion with it, and converting what at first sight was regarded as contradictory, into the firmest arguments for its truth.

VIII. *On the physical cause of Fraunhofer's dark lines.*

Although I have extended this memoir to so great a length, I have omitted many facts which have been made the subject of experiment. I cannot however conclude without offering some remarks on the artificial production and cause of Fraunhofer's fixed lines.

It has already been related how I was led to expect the production of these lines in the flame of cyanogen from considering the circumstances under which its combustion takes place. Returning to this phenomenon, I shall here point out a very remarkable numerical relation existing among the fixed lines of the solar spectrum.

The following table contains Fraunhofer's determinations of the wave lengths corresponding to the seven great fixed lines of the spectrum, which are designated by the capital letters of the alphabet from B to H. I have added the wave length of A from my own experiments.

Table of wave lengths, corresponding to the eight great fixed lines of the solar spectrum; the Paris inch being supposed to be divided into one hundred millions of equal parts:

A = 2660	E = 1945
B = 2541	F = 1794
C = 2422	G = 1587
D = 2175	H = 1464

An examination of this table proves,

the length of	B	is	119	parts	less	than	A.
"	"	C	"	238	"	"	"	"
"	"	D	"	485	"	"	"	"
"	"	E	"	715	"	"	"	"
"	"	F	"	866	"	"	"	"
"	"	G	"	1073	"	"	"	"
"	"	H	"	1196	"	"	"	"

and these differences of length are obviously very nearly as the whole numbers 1, 2, 4, 6, 7, 9, 10. This coincidence is far too striking to be merely accidental. Moreover, it must not be forgotten, that the observed numbers as determined by Fraunhofer are wholly independent of any hypothesis.

If the relation of whole numbers was rigorously true, the numbers in the foregoing table would stand as follows: 119, 238, 476, 714, 833, 1071, 1190.

The wave length of the most luminous portion of the spectrum, the centre of the yellow space, is 2060 parts. If we take

this as an optical centre, it will be found that the great lines are situated symmetrically in relation to it. E and D are equidistant above and below it; the same observation applies to G and B; and also to H and A. The only departure from this symmetry is in the case of F, which is not symmetrical with C. It will be understood that I am here speaking of one of those spectra which are formed when a grating or ruled surface is used. In this the colors are arranged side by side, according to their wave lengths; the centre of the spectrum which is its most luminous portion, is occupied by the centre of the yellow space; and the light terminates at equal distances in the violet and red.

Do not these observations lead us to conclude that the cause, whatever it may be, that produces these fixed lines is periodic in its action?

What that cause in reality is, we have not now facts sufficient to determine. I would not affirm that the disengagement of incombustible matter by a flame will always give rise to dark lines. But this is very clear, that in all those cases, as cyanogen, alcoholic solutions of nitrate of strontian, of boracic acid, &c., in which these lines are developed, incombustible matter is uniformly disengaged.

Art. XIX.—*Caricography;* by C. Dewey, D.D.

(Appendix, continued from vol. iv, ii Ser., p. 350.)

No. 227. *C. lagopina*, Wahl., No. 37; Tor. Mon., p. 393.—*C. leporina*, Willd. and Good., (not of Lin.)—*C. Sachenalii*, Schk. Schk., Tab. Y, fig. 79 and Tab. Fff, fig. 129.

Spica composita; spiculis ternis (2–4) confertis inferne staminiferis alternis ovatis vel ellipticis; fructibus *distigmaticis* ellipticis convexo-subconcavis rostellatis vel acuminatis margine obtusis ore bilabiatis, squama ovata acuta paulo longioribus.

Culm 4–8 inches high, erect, triquetrous, leafy towards the base; leaves linear, flat, shorter than the culm; spikelets commonly three (2–4), ovate and elliptic, aggregated, close-fruited, staminate at the base; stigmas two; fruit ovate, convex above, and often concave on the under side, smooth, obtuse on the margin, acuminate, often short rostrate or apiculate, of variable length, orifice entire or slightly two lobed; pistillate scale ovate, acutish, white and scarious on the edge, sometimes not much exceeding half the length of the fruit.

Rocky Mountains, *Drummond*, and agrees with the well known plant of the Alps, Norway, &c. By Schk., two plants of different names were properly united under *C. leporina*, Willd.

The *C. leporina*, L., is *C. ovalis*, Good., described in this Journal, vol. xxvi, p. 377, and in vol. vii, p. 276, and entirely different from *C. lagopina*, Wahl. *C. leporina*, L., much resembles our *C. scoparia*, Schk., from which however it differs in its broader or far less lanceolate fruit and scale.

Specimens of this plant and the following three were received through the politeness and kindness of Dr. Torrey.

No. 228. *C. cryptocarpa*, Meyer.—*C. Scouleri*, Tor. Mon. Cyp., p. 399. Boott in Hook. Flor. Bor. Am., No. 88.

Spicis staminiferis binis pedunculatis gracilibus utrinque teretibus; spicis pistilliferis ternis oblongis cylindraceis recurvo-pedunculatis bracteatis evaginatis densifloris, inferiore longo-pedunculatis; fructibus *distigmaticis* orbiculatis convexis brevi-apiculatis striatis ore integris, squama ovata ferruginea acuta paulo brevioribus.

Culm a foot or more high, slender, triquetrous, smooth, with short leaves towards the root, and with linear bracts without sheaths; staminate spikes two, half inch long, pedunculate, with wide and oblong obtusish scales; pistillate spikes three, oblong and cylindric, upper one with staminate scales at the apex, all on recurved peduncles and the lowest long pedunculate; stigmas two; fruit elliptic or roundish, lenticular, short apiculate, striate or minutely nerved; pistillate scale ovate, acute, dark colored, longer than the fruit.

Northwest coast, *Dr. Scouler*, and Sitcha, *Bongard*. Resembles *C. frigida* and *C. limosa* in general aspect, but is very different; belongs in another section. Dr. Boott notices its resemblance to one variety of *C. salina*.

Note.—C. Sitchensis, Prescott, was described in vol. xxix, p. 245, under the name of *C. cryptocarpa*, but is a very different plant from this.

No. 229. *C. Jamesii*, Tor. Mon. Cyp., p. 398, (not of Schw. An. Tab.)

Spicis staminiferis binis oblongis crassis approximatis; pistilliferis ternis oblongo-cylindraceis densifloris, superioribus approximatis brevi-pedunculatis evaginatis, infima perlongo-pedunculata foliaceo-bracteata; fructibus *distigmaticis* ovatis convexis subinflatis nervosis brevi-rostratis ore bilabiatis, squamam ovatam acutam vel brevi-mucronatam paulo superantibus.

Culm half a foot high, thick, obtusely triquetrous, with leafy bracts and radical leaves shorter than the culm; staminate spikes two, lower short and close to the other, upper one an inch long, large and thick, with oblong and obtuse black scales; pistillate spikes three, cylindric, bracteate, two upper on short peduncles

without sheaths, the lowest with a long and nearly radical peduncle; stigmas two; fruit ovate, convex, short rostrate, nerved, orifice bilabiate; pistillate scale ovate, acute, shorter than the fruit.

Rocky Mountains, *Dr. E. James*, and named by Dr. Torrey in honor of the discoverer. It seems to be a fine species.

No. 230. *C. leiocarpa*, Meyer. Tor. Mon. Cyp., p. 387. Boott in Hook. Fl. Bor. Am., No. 2.

Spica unica oblonga laxiflora superne staminifera, vel dioica? fructibus *tristigmaticis* oblongis lanceolatis distincte triquetris ore integris vel sublabiatis, squama oblonga patula obtusa paulo longioribus.

Culm a foot or more high, slender, erect, triquetrous, leafy towards the base; leaves narrow, linear, triquetrous at the apex; spike single, oblong, not closely fruited, towards an inch long, staminiferous at the summit; stigmas three; fruit oblong, distinctly triquetrous, tapering above, smooth (as its name signifies), with its orifice entire or very slightly two-lobed; pistillate scale oblong, obtuse, brown, a little shorter than the fruit.

In Russian America, *Meyer* and *Bongard;* resembles *C. polytrichoides*, but very different. I find three stigmas on the fruit.

No. 231. *C. microdonta*, Torrey and Hook. Tor. Mon. Cyp., No. 145, and Boott in Bost. Journ.

Spicis staminiferis subternis (2–4) approximatis cylindraceis sessilibus, infima subremota inclusè pedunculata; spicis pistilliferis 3–4 sæpe quatuor cylindraceis erectis exsertè pedunculatis subremotis bracteatis, superioribus apice staminiferis supernè attenuatis basi sublaxifloris; fructibus *tristigmaticis* ovato-oblongis compressis striatis ore subemarginatis, squamam ovatam latam cuspidatam vel acuminatam subæquantibus.

Culm 1–2 feet high, slender, obtusely triquetrous, leafy towards the base, and leaves shorter than the culm; staminate spikes about three, cylindric, from a half to an inch and a half long, upper ones near, the lowest exsertly pedunculate and remotish, with lanceolate and cuspidate scales; pistillate spikes about four, remote, cylindric, acutish above by staminate flowers with exsert peduncles; *stigmas three;* fruit ovate, oblong, obtusish with an emarginate orifice and with ovate cuspidate scales scarious on the margin.

Texas, *Drummond;* Torrey and Boott.

No. 232. *C. microglochin*, Wahl. Schk., Tab. Ssss, fig. 210. Boott in Hook. Flor. Bor. Am., No. 15.

Spica unica apice staminifera densi-flora; fructibus lanceolatis conicis convexo-subplanis reflexis apice aristatis vel subulatis *tristigmaticis* subocto-floris cum squama caduca.

Culm a few inches high, slender, with setaceous leaves towards the base; spike single, staminate above, and close flowered; stigmas three; fruit conic, lanceolate, subconvex-plane, subulate at the apex and reflexed; pistillate scale caducous.

Greenland, *Horneman;* Switzerland, *Schk.*, who distinguishes it from *C. pauciflora*, especially by the subulate point beyond the orifice at which the style passes out.

No. 233. *C. lenticularis*, Mx. Boott in Hook. Fl. Bor. Am., No. 85.

Spicis distinctis; staminifera unica; spicis pistilliferis oblongis pedunculatis pluribus; fructibus lenticularibus brevi-ovatis muticis; foliis acuto-longis, culmo triquetro subæqualibus.

Canada to Hudson's Bay, *Michaux;* "Canada to Mackensie's River, *Richardson*," says Dr. Boott, who also states that the terminal spike is often staminiferous. The resemblance to *C. panicea*, L., is remarked by Mx.; and hence it is evident that this plant is not well known by our botanists, although it is found over a wide extent of the northern regions. The above given description is really that of Mx., and may lead to a better acquaintance with it. It is not the *C. lenticularis* in vol. vii, p. 273, which proves to be a variety of *C. limosa*, L. It has been supposed to be a variety of *C. stricta*, which its affinity to *C. panicea* forbids.

No. 234. *C. recta*, Boott. Hook. Fl. Bor. Am., No. 95.

Spicis staminiferis binis oblongis, superiore sessili; spicis pistilliferis subternis (3–4) cylindraceis erectis approximatis foliaceo-bracteatis evaginatis pedunculatis, superioribus superne staminiferis, infima subremota inferne laxiflora; fructibus *distigmaticis* obovatis subrotundis lævibus brevi-rostratis ore integris vel subemarginatis, squamam lanceolatam inferne aristatam semi-æquantibus.

Culm 18 inches high, erect, stiff, scabrous on edges above, longer than the close lanceolate leaves; bracts leafy, longer than the culm, auriculate at base and not sheathing; staminate spikes two, near the pistillate and erect, with dark brown scales above obtuse; pistillate spikes 3–4, oblong, cylindric, erect and close, near, upper ones staminate at the apex, the lowest remotish; stigmas two; fruit roundish obovate, glaucous, very smooth, with a round and short beak and orifice nearly entire; pistillate scales lanceolate, dark-purple, awned below, and longer than the fruit nearly twice. *Boott.*

Labrador, *Hooker.* This species belongs with *C. acuta* and *C. stricta*, and is well characterized.

Note.—*C. oligocarpa*, Schk., described in vol. iv, p. 349, has one form, which has usually been ranked under *C. Hitchcockiana*,

found abundantly by Dr. Sartwell of Penn Yan, and named by Gay as distinct, *C. Sartwelliana*. But Dr. Gray ascertained it to belong to *C. oligocarpa*, of which it seems to be a manifest variety and to deserve the following designation.

C. oligocarpa, Schk.; var. *Sartwelliana*, D.;—*C. Sartwelliana*, Gay.

Fruit subovate, oblong, roundish triquetrous, tapering above or short-conic rostrate, about equalling or surpassing the short, ovate, obtuse and long-awned scale.

The fruit of *C. Hitchcockiana* is tapering at both ends; this is subovate, and is far less obovate and more tapering at the apex than that of *C. oligocarpa*.

New York and western states—common.

ART. XX.—*On the Parallelism of the Palæozoic Deposits of North America, with those of Europe; followed by a Table of the Species of Fossils common to the two Continents, with indication of the positions in which they occur, and terminated by a critical examination of each of these species;* by ED. DE VERNEUIL, (translated and condensed from the Bulletin of the Geol. Soc. of France, 2d ser., vol. iv,* for this Journal; by Mr. JAMES HALL, New York State Geologist)

"AFTER having studied, during many years, in Russia and in Sweden the distribution of the fossils of the Palæozoic strata, the principal object of my short visit into North America, was to ascertain if the stratigraphical distribution of animals, is the same there as in Europe. It was therefore necessary to become familiar first with the geology of this immense country, by collecting from each bed the different fossils,† or at least the principal ones, in order to distinguish, 1st, the species proper to America; 2d, the species identical with those of Europe. The first point will be left for a subsequent work, in which we shall occupy ourselves farther with the geology of the United States; the second, will form the subject of the following remarks, which I shall endeavor to restrict within limits appropriate to the Bulletin."

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* Sur le parallélisme des depots paléozoiques de l'Amerique Septentrionale avec ceux de l'Europe; suivie d'un tableau des espèces fossiles communes aux deux continents, avec l'indication des étages où elles se rencontrent, et terminée par un examen critique de chacune de ces espèces.

† "In describing the new species which we have brought with us, we shall often have occasion to render justice to the different scientific men who with so much success are occupied with the geology of the states which they inhabit, and to whom we owe a good part of our fossils. We beg them, in the mean time, to accept here the expressions of our gratitude."

"To render intelligible the distribution of fossils in America, and enable our readers to judge if the species identical with those of Europe are found in the same beds as these last, if in fine the epoch of their appearance, and the period of their duration, are the same on both continents, we will make known, as briefly as possible, the series of deposits which compose the Palæozoic strata, in the state of New York, in that of Ohio, and in the other states situated on the northwest of the Alleghanies. The superposition of the beds and the groups once well established, we will consider what are the species they contain identical with those of Europe. It is there that we find the elements necessary to a solution of this double question, 1st, do the species present themselves in the same order in America as in Europe, or in other words, is there in the succession from the most ancient fossiliferous beds to the coal period inclusive, a law common to the two continents? 2nd, is it possible to trace, among the different groups of the Palæozoic strata of America, divisions corresponding to those which we have established on this side of the Atlantic, under the names of Silurian, Devonian and Carboniferous systems? The importance, which in these researches, we are induced to accord to the species common to the two continents, has induced us to give the list of them with the divisions in which they are found, and to give to this table some details upon each of these species, and in particular upon their different positions.

Composition of the Palæozoic strata in the state of New York. —In the present state of our knowledge, there is perhaps no series of formations which is extended in a continuous manner over a vaster surface than the Palæozoic strata of North America, and that alone is sufficient to render it worthy of our interest. The parts of this immense basin already known, comprise from thirty to thirty-five degrees of longitude, and fifteen degrees of latitude; but its northern limits are still uncertain, and if, as some travellers believe, they advance even towards the polar circle, the Palæozoic formation embraces, from Tuscaloosa in Alabama, its southern extremity, a space of more than thirty degrees of latitude. All this extent is far from having been equally studied. Many states have ordered geological examinations destined to make known the mineral riches of their soil, and which have given rise to very valuable publications; but the state of New York has done more, she has undertaken the noble task of publishing a complete work upon her Natural History. The geological labors confided to eminent men, have thrown a brilliant light upon the geological structure of this part of the United States. By one of those happy chances of which the history of science offers us examples, the territory of the state of New York presents us, below the carboniferous system, the Palæozoic series most complete. Every

favorable condition is there also united to facilitate stratigraphical studies, and to give to superposition, and consequently to Palæontology, *of which it is the foundation*, a certainty truly scientific. Indeed, in the northwest part of the state, the beds incline very gradually to the south, in such a manner, that in advancing towards the north the more ancient beds successively appear at the surface. The outline of the surface presents an inclination precisely the reverse; it attains its greatest elevation near the limits of Pennsylvania and declines to the north towards Lake Ontario. There results from this condition a series of lakes and little rivers, which, flowing from south to north, cut the formations transversely. These streams, profiting, probably, by preëxisting fissures, indent the surface, as the Niagara, to the depth of two hundred or three hundred feet, and allow the examination and study of the superposition of the successive strata with certainty. We have had occasion more than once to compare the natural advantages of this country, with the difficulties presented by Russia in Europe, where the plains are so little broken, and the thick diluvial mantle which covers them scarcely rent.

No country in Europe offers us so complete and uninterrupted a development of the Silurian and Devonian systems; to observe it, it is necessary to cross the Atlantic. From the most ancient fossiliferous beds which correspond to the lower strata of the Silurian system, to the coal period exclusively, there is in the state of New York, a continuous succession of deposits which are superimposed in regular stratification without however entirely covering each other. The four official geologists of the state of New York are agreed in acknowledging twenty-eight groups or subdivisions which they have united in the following manner."

These introductory remarks of M. de Verneuil, which give the reader an idea of the scope and object of the paper, are followed by a table of the groups and individual rocks of the Palæozoic series in the state of New York, and a short description of each one in the ascending order. The characteristic fossils of each group or rock are also indicated, being to a great extent those already given in the Geological Reports of New York, and which have likewise appeared in this Journal, in connexion with a review of the same works.

The order of the successive groups of this series is now so generally known, that it does not appear necessary to repeat the table in this place. M. de Verneuil remarks with justice that these groups are of unequal value, some of them being but a few feet in thickness, while others are many hundreds. This has already been acknowledged in the published reports, and though it may now be easy to make a more perfect grouping, uniting individual rocks which in the table have the same value as groups, yet it was not an easy task to do this before the publication of the re-

sults of the several observers in different parts of the state. He remarks also that the grouping of all these in divisions, is less happy, alluding to the Champlain, Ontario, Helderberg and Erie divisions. This arrangement was followed in some of the reports, as a matter of concession, and it is to be regretted that such an artificial classification was ever, for a moment adopted, although only with a wish of giving unity to the several reports. All arrangements of this kind, indicating geographical limits or locations, are of very trifling importance when compared with those founded on Palæozoic characters. In the present instance, the first division embraces all the lower strata to a very natural horizon, while the second includes only a part of a natural group, the upper portion of which is carried into the third division. The third division includes a portion of two natural groups, but embracing neither of them entirely. It is to be regretted that these divisions are perpetuated in any publication, since at the time of their adoption, they were assumed merely as geographical subdivisions, and regarded as of no more value than to indicate the region or neighborhood, in which certain rocks or groups occupied the surface and surrounding country.

M. de Verneuil proposes a union of several of the smaller groups; in which we agree with him, having already adopted them in the palæontological arrangement. Notwithstanding this, we are still disposed to retain all the names given, rather than to merge any of them in more general terms; except for the expression of a series which shall unite several minor beds, all of which are characterized by certain zoological features. In truth we are satisfied that what has given certainty and security in our labors, are the minute subdivisions which have never been attempted elsewhere. It is easy to designate great groups, but that such groups shall be permanent, it requires that their elementary parts shall have been studied with care. It is much easier to unite the minor subdivisions in one great whole, than to separate any of the larger groups now recognized into well characterized smaller parts.

Commencing the descriptions of the rocks with the Potsdam sandstone, which he regards as the analogue of that sandstone of Russia and Sweden, which lies at the base of the sedimentary formations, our author continues his descriptions of the series in the stratigraphical order. Regarding the calciferous sandstone as a distinct mass, he follows with some reason also the arrangement adopted in the New York reports, of uniting the Chazy, Birdseye and Black River limestones in one group; though each possesses peculiar fossils, and, where fully developed, are in fact very slightly connected by their organic remains. Still in western localities there appears to be less positive means of separating them.*

* These rocks contain together about eighty species. Of these only two or three known in the Chazy, are found in a higher position, and a single species of the

There is very good reason for uniting the Trenton limestone, Utica slate, and Hudson River group; for although the middle term contains few fossils of any species, yet several are common to the three terms, and of those known in the Trenton limestone, twenty species reappear in the Hudson River group. M. de Verneuil has become satisfied of the fact, that these groups form but a single one at the west, scarcely distinguishable into the three divisions which are well marked in New York. Notwithstanding there are in the Hudson River group several species of Gasteropoda and Acephala, quite distinct from those of the Trenton limestone, yet the species of Brachiopods are identical;—not more than two species appearing at this period which are not also common to the Trenton limestone.

In the description of the succeeding rocks, M. de Verneuil very properly suggests that the Oneida conglomerate and grey sandstone should form the base of the succeeding division, rather than remain united with the Hudson River group in the Champlain division. This course has already been followed in the Palæontological report,—the first great division being regarded as ending with the Hudson River group, at which point the greater part of those species regarded as belonging to the lower Silurian epoch, terminate their existence.

It is not necessary to follow our author through all the successive groups, since the principal object is to arrive at his results. In the second period of our system, he suggests a union of the water-lime, the *Pentamerus galeatus* limestone, the *Delthyris* shaly limestone, and upper *Pentamerus* limestone, as one group. In this not only do we readily acquiesce, but are happy to be sustained in what we have already done in effect, by so able and impartial an observer as M. de Verneuil. These subdivisions are no where perfectly discernible except in the eastern part of New York, and here it is quite evident that they are united by their organic contents, although slightly different in lithological character, and each bed showing a predominance of certain forms, to the almost entire exclusion of all others. Our author remarks, that the lower of these four groups rests upon the Hudson River group, at Schoharie, which, though generally very nearly true, is not precisely so, there being a partial representation of both the Onondaga salt group and Niagara group, as we have had occasion to show elsewhere.

M. de Verneuil proposes to unite the Caudagalli grit with Schoharie grit, and the Onondaga with the corniferous limestones. A more natural arrangement, and one which the organic contents compel us to follow, is to unite the Schoharie grit, the Onondaga

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Birdseye passes into the Black River limestone; while an equal number from both are known in the Trenton period.

and corniferous limestones, all these having fossils in common, and only in particular localities distinguishable into these subdivisions. On the other hand, the Caudagalli grit commences with the termination of the Oriskany sandstone, contains no fossils except the peculiar "enigmatical" one giving it the name, is coëxtensive in New York with the Oriskany sandstone, and does not appear beyond its western termination, while the other rocks are continued.*

In proceeding, our author proposes to unite the Marcellus shale, Hamilton group, Tully limestone and Genesee slate in one division, and make the Portage and Chemung the second of this period. In this we quite agree, admitting that such an arrangement is most conformable to the Palæozoic indications.

We are happy to observe in the concluding remarks of M. de Verneuil, that he has attached due importance to the difference in the character of the sediment in the eastern extension of the formations, as indicating the proximity of shores; and also the occurrence of fucoids and ripple marks, indicating always a shallow sea. We give below the passage:

"If we embrace in a single view, the Palæozoic formation of the State of New York, we may observe some general features which we will merely indicate here, before passing to the study of the same formation in the States of the west. One important fact which strikes the observer, is that the beds being horizontal and conformable, the organized creatures present under the single influence of time, such successive modifications that the greater part of the groups or subdivisions, are characterized by species proper to the different beds of which they are composed, and by others less numerous, which connect them with the superior and inferior groups. The disposition of the mineral materials also gives place to important considerations. The predominance of the sandstones and the schists in the eastern part of the state of New York, seems to indicate that on this side existed the continent, whose streams and shores furnished to the sediments the elements of which they are composed.† This view has not es-

* We are extremely sorry to see that M. de Verneuil cites the occurrence of Catenipora at Caledonia, in Livingston county, on the authority of Col. Jewett, of Lockport. This is a good locality of the corniferous limestone, and we are quite confident that a Catenipora could never have been found there. I have a specimen from Col. Jewett labeled from the Niagara limestone at Lockport, and containing *Orthis testudinaria*, *Chætetes lycoperdon*, and the peculiar Tentaculites of the Trenton limestone. Unfortunately our country has too many such collectors, who attach no importance to the locality of a specimen, and when trusted at all, produce the most serious confusion. Nothing is of so much importance relative to the subject of palæontology and geology, as that of ascertaining the true locality and position of specimens, and we cannot too severely reprehend the practice of giving or of selling specimens as from one locality, which are from a different and distant one.

† How is it that this great continent has not furnished to the sediments formed upon its shore, the remains of terrestrial animals and vegetables? This cannot fail to be matter of astonishment. The first terrestrial plants, analogous to those

caped the American geologists, and that which still farther confirms it, is the occurrence of the fucoids and ripple marks, with which the surface of the beds is often covered; unquestionable evidence of the vicinity of shallow waters (*bas-fonds*, low bottoms) and of shores. An important circumstance is that these fucoids and ripple marks are observed at all stages, from the Potsdam sandstone to the Portage group; so that it becomes incontestible that the whole mass of the Palæozoic formation, however thick, has been deposited in a shallow sea, of which the bottom probably subsided successively, to receive new sediments. What becomes then of the opinion of certain geologists, who think that all the Silurian deposits have been made in very deep seas, and who attribute to this circumstance, rather than to the action of time, the differences which distinguish the Silurian fauna from those which have followed?"

"*Composition of the Palæozoic formation in the states of Ohio, Kentucky, and Indiana.*—Between the great coal basin which occupies a part of Pennsylvania and of Ohio, and that of Illinois, rises an anticlinal axis which separates them, and which elevates all the more ancient beds even to the lower stage of the Silurian system. It is not an axis of entire dislocation; the beds continue to remain apparently horizontal, their dip being only fifteen or sixteen feet in the kilometer, but this dip being in opposite directions on the two sides of a line nearly north and south, which passes by Cincinnati, is sufficient to bring to light throughout a great extent, in wide bands, the inferior parts of the Palæozoic strata. Nothing is more interesting, when once familiar with the rocks and the fossils in the state of New York, than to transport one's self one hundred and fifty, or two hundred leagues to the west to observe the changes which this series presents, and to seek for the cause which we will do in the briefest manner."*

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of the carboniferous period, appear only in the Chemung group, and these are very rare.

* "We recommend persons who desire more details on this subject, to read the two very interesting memoirs of Mr. J. Hall, one upon the identity of the formations of the west in the United States, with those of the state of New York, printed in the Transactions of the American Association of Geologists, &c.; the other upon the nature of the strata and the distribution of fossils in the ancient formations of the United States, inserted in the Journal of the Boston Society of Nat. Hist., vol. v. We have had occasion to recognize the exactness of the observations of this able geologist, and we only differ from him in opinion upon the age and the true equivalents of the black bituminous shales, and of the principal mass of the micaceous sandstones which overlie them in the states of Ohio, Indiana and Kentucky."†

† M. de Verneuil is probably correct regarding the black bituminous shale; but between this rock and the knobs near New Albany, there is a thickness of four or five hundred feet, the lower part of which is shaly, of a greenish color, and marked by fucoids and casts of mud furrows; in fact, undistinguishable from the shaly sandstones and shales of Portage, and never marked by the presence of Productus and other fossils which mark the yellowish sandstones of a higher position, and which were denominated in the section as "sub-carboniferous rocks." This point, however, we do not regard as of very essential importance, now that we understand the true succession of these strata and their identity with eastern types.

"*Table of the groups which compose the Palæozoic formations in the states of Ohio, Kentucky and Indiana.*"*

Carboniferous system,	8. Sandstones, shales and limestones of the coal.
	7. Carboniferous limestone.
	6. Fine grained, micaceous sandstone.
Devonian system,	5. Black, bituminous schists.
	4. Shelly limestone, and upper coralline limestone.
Silurian system, (superior,)	3. Siliceous or magnesian limestone, (cliff limestone.)
Silurian system, (inferior,)	2. Blue limestones and marls.
	1. Compact limestone.

The character of these formations in detail is already known to most readers of this Journal through the Reports on the Geology of the different States, and from the Review of the N. Y. Geol. Reports and comparison of Western formations, by D. D. Owen.

M. de Verneuil concludes his descriptions of these rocks by some observations upon the simple geological structure of the Western States as compared to New York. All the schistose and arenaceous deposits below the carboniferous period have disappeared almost entirely, even the extensive and important groups of Hudson River, Hamilton and Chemung are scarcely recognized in their western extension, and the old red sandstone is entirely wanting. On the other hand, the principal calcareous formations have augmented their thickness and importance. These schistose formations being influenced by the proximity of shores and currents, are less reliable than calcareous formations in studying the succession of strata. For these reasons, M. de Verneuil remarks, that in making a comparison of the strata of the two continents the calcareous formations are to be regarded as the most important.

In terminating this sketch, he remarks how much the predominance of limestone towards the base of the system contrasts with the composition of the same strata in England, where according to the observations of Prof. Sedgwick, all the bands of limestone below the carboniferous series are purely local phenomena, which appear only at intervals. This observation upon the predominance of limestones in the United States, is strikingly true, since we find that each of the three important periods below the carboniferous, is marked by a calcareous formation extending over many thousand miles; recognizable at numerous points between the Atlantic and the Mississippi, and in greater or less perfection reaching even the opposite side of the continent.

This sketch of the Palæozoic strata of the United States, is followed by a comparison with the strata of the same age in Europe, which we shall give entire.

(To be continued.)

* "It is to Messrs. R. Dale Owen, Mather, Locke, Hildreth and Foster, that we owe our knowledge of the geological constitution of these States."

ART. XXI.—*On the Reaction of Solid Carbonic Acid with Alkaline and other Bases;* by WM. F. CHANNING, M.D.

1. THE production of heat by the reaction of solid carbonic acid with a caustic alkali, was noticed by me in this Journal for January, 1844. A more precise series of observations on the same reaction, extended to a greater number of bases, will be found to afford some additional facts in the history of the acid, and to illustrate a most striking instance of the evolution of heat by chemical union. The details of manipulation will be preserved in the following experiments, chiefly to indicate the best mode of exhibiting the phenomena described.

2. Solid carbonic acid was placed in contact successively with different bases, presented to it for the most part in the dry state. The intensity of the reaction and the amount of heat produced was generally estimated by the effects of ignition and fusion obtained. The heat being local, and one of the substances present being in a state of intense refrigeration, it was found that no accurate measurement could be made by employing a thermometer either in contact or subjected to radiation.

3. A small fragment of solid carbonic acid, was comminuted as much as possible, and placed on cotton wool. A little pure pulverized hydrate of potash was poured upon it. A piece of gun-cotton was laid over these, the whole covered with cotton wool, and compressed by a spatula. In a few seconds the gun-cotton exploded. Gun-cotton compressed with either carbonic acid or hydrate of potash separately, showed no disposition to ignite.

4. Pure hydrate of soda was substituted for hydrate of potash with a similar result, acting apparently with equal intensity. The pure hydrates of soda and potash, evaporated in silver vessels, contain frequently some additional water to the one atom belonging to the hydrate, which interferes with the above reaction. Thus if the cylinders of common hydrate of potash, which is evaporated at a higher heat in iron vessels, are employed, the reaction is more intense and the result more immediate. That this is not occasioned by the presence of carbonate of potash, existing as an impurity in the hydrate, is shown by subjecting protocarbonate of potash itself to reaction with the carbonic acid. In this case no sensible heat is evolved. The energetic reaction of solid carbonic acid with the alkalies, seems to terminate with the formation of protocarbonate. Pure potassa was also mixed with quicklime, and then treated with the carbonic acid, but without any increase of action.

5. The alkaline earths were heated to redness in a platina capsule before each experiment, and employed as soon as cooled. Anhydrous lime exposed to solid carbonic acid, gave no indica-

tions of heat or reaction. The lime was then carefully hydrated by adding drops of water. Subjected to solid carbonic acid, reaction now took place, but the heat was sensibly less than when hydrate of potash was employed. Gun-cotton remained unignited. A little of the hydrate of lime was placed upon tinfoil. A piece of solid carbonic acid, flattened by the spatula, was placed upon the hydrate of lime and covered with an additional quantity of the same. The tinfoil was then wrapped up and compressed. The heat evolved was too great to be borne by the hand, and caused a fragment of phosphorus placed upon the foil to ignite.

6. Anhydrous baryta, strontia and magnesia gave no reaction with the carbonic acid. Their hydrates all produced heat, slightly less perhaps than that evolved by hydrate of lime.

7. It might have been anticipated that the reaction of the alkaline earths with solid carbonic acid, would be more energetic than that of the alkalies from their habitudes in solution. But the decisive fact of the superior affinity of the alkalies for carbonic acid in the dry state, would lead us to infer that the result in solution is due to the "influence of insolubility" in the precipitated carbonates of the alkaline earths.

8. It seemed desirable to extend the inference that carbonic acid does not combine directly with an anhydrous base. Potassium was therefore cut into thin layers and allowed to oxydate in the air. From the great disposition however of solid carbonic acid to condense moisture on itself, and the liability of the mingled potash and potassium to ignite spontaneously or by slight causes, no accurate result could be obtained. There was no evidence of the production of heat by any reaction with the carbonic acid.

9. The anhydrous protoxyds of lead, zinc and copper were subjected to solid carbonic acid placed upon cotton. No elevation of temperature could be detected. The hydrates of these oxyds, when wrapped in cotton with carbonic acid and compressed by the fingers, gave a doubtful result to the sensation, the temperature being very slightly elevated if at all.

10. Solid carbonic acid wrapped in gun-cotton was exposed in the open end of a glass tube to dry ammoniacal gas generated at the closed end, and desiccated by passing through a portion of quicklime in the centre of the tube. No striking result was produced, but a sensible heat was obtained, especially at the close of the experiment, when a portion of moisture probably came over with the ammonia. In place of the solid carbonic acid employed, a piece of carbonate of ammonia was found occupying its place in the cotton after the experiment. Gun-cotton placed alone in the tube, gave no such reaction. The gun-cotton which had been exposed to the gaseous ammonia, it may be remarked, was observed to burn more slowly and with a red flame.

11. Solid carbonic acid was compressed in contact with hydrated acetate of lead, a salt readily attacked by carbonic acid, but no sensible heat was evolved.

12. To obtain some indication of the amount of heat which could be produced by this reaction, common hydrate of potash was pulverized and spread upon tinfoil, a flattened piece of solid carbonic acid placed upon it, and covered again with hydrate of potash. The whole being compressed gave a heat estimated to be near the melting point of tin, though actual fusion could not be effected by any arrangement. A splinter of pine brought in contact with the foil was slightly discolored, and gave the characteristic smell of charred wood. The temperature may therefore be safely estimated at between 350° and 400° F. By substituting a slip of platina foil above and below the reagents, in place of the tinfoil, ether, alcohol and water dropped upon it, were successively vaporized. Phosphorus was instantly inflamed on touching the foil. A piece of fusible metal placed upon the foil was melted.

13. Hydrate of potash and solid carbonic acid were ground together in a small mortar. Sufficient heat was produced to explode gun-cotton, but the experiment is an undesirable one for repetition. The experiment with gun-cotton in §3 may be diversified by placing a portion of cotton wet with alcohol, near but not in contact with the gun-cotton. The alcohol is then ignited by the explosion of the latter.

14. The reaction of solid carbonic acid with liquids and solutions, is referable, to some extent, to the same principle as its reaction with bases in the dry state. When immersed in water, it combines with this latter, and at the same time the evolution of gas is so rapid as to prevent the solid from touching the liquid. Owing to these causes the temperature of the water falls only a few degrees, instead of the anticipated result of immediate congelation, and the thermometer held in the gas escaping from the surface indicates a temperature slightly above that of the surrounding air, this heat being the heat of combination, evolved at the point of union of the carbonic acid and the water, acting as a base. With a solution of potash the reaction is very much the same, the only observable point being the great quantity of gas which escapes absorption.

15. When solid carbonic acid is treated with ether or alcohol, it assumes a semi-fluid state, and no chemical combination probably taking place, and the vaporization of the acid being augmented, these mixtures become of the most frigorific character, and are commonly used for this purpose. When however hydrate of potash is added to these in a mortar, the mixture with ether rises to a temperature probably above 40° F., and that with alcohol still higher, with occasional violent ebullition.

16. Taking the temperature of solid carbonic acid at $-148°$ F., and the greatest heat obtained by its reaction with the hydrated alkalies at from 350° to 400° F., there is an elevation of temperature produced in this case of more than 500° F. As this scale goes far below as well as above the natural temperature, it affords an opportunity of exhibiting the effects of intense cold and intense heat side by side and proceeding from the same substance. These experiments have therefore an illustrative value in connection with heat and chemical affinity, as well as with the properties of carbonic acid in the solid state.

Boston, Dec. 20th, 1847.

ART. XXII.—*Historical Notice of the Discovery of the Planet Neptune;* by Professor LOOMIS, of the New York University.

THE discovery of the planet Neptune, Sept. 23, 1846, at Berlin, was announced in this Journal, vol. ii, p. 439; and various particulars respecting it have been given in the Numbers for the past year. As however the general reader might fail to obtain from these detached notices a just idea of the connexion of the several links of this most important discovery, it is now proposed to place on record a concise history of this planet.

The discovery of Neptune resulted from the study of the motions of the planet Uranus. Uranus was first discovered to be a planet in 1781, but it had been repeatedly observed before by different astronomers, and mistaken for a fixed star. Nineteen observations of this description are on record, one of them dating as far back as 1690. In 1821, M. Bouvard of Paris, published a set of tables for computing the place of this planet. The materials for the construction of these tables, consisted of forty years' regular observations at Greenwich and Paris since 1781, and the nineteen accidental observations, reaching back almost a century further. Upon comparing these observations, Bouvard found unexpected difficulties. It was impossible to combine all the observations in one elliptic orbit. When he attempted to unite the ancient with the modern observations, the former might be tolerably well represented, but the latter exhibited discordances too great to be ascribed to errors of observation. Not being able satisfactorily to explain this discrepancy, he rejected the ancient observations, and founded his tables upon the observations since 1781. These tables represent very well the observations of those forty years; but soon after 1821, new errors began to appear, which have gone on increasing to the present time. In five years, the discordance between the observed and computed place of the planet became noticeable; in ten years the error had amounted to

half a minute of space; and now the error exceeds two minutes. This is equal to one fifteenth part of the apparent diameter of the sun or moon, which though small in itself, is large when compared with the precision of modern observations. What could be the cause of these discrepancies? Were the tables computed inaccurately? The errors were too large, and Bouvard was too skillful a computer, to permit such an explanation. Were these discrepancies due to the action of some unknown disturbing body? This idea was seriously entertained more than twelve years ago by Bouvard, Hansen, Hussey, and some others. Mr. Hussey even proposed to compute an approximate place of the supposed body, and then commence searching for it with his large reflector. Mr. Airy, now Astronomer Royal of Great Britain, at that time Professor in Cambridge, pronounced the problem hopeless. His words were: "If it were certain that there was any extraneous action upon Uranus, I doubt much the possibility of determining the place of the planet which produced it. *I am sure it could not be done, till the nature of the irregularity was well determined from several successive revolutions;*" that is, till after the lapse of several centuries.

This deliberate opinion from one who by common consent stood at the head of British mathematicians and astronomers, would have deterred any but the most daring mathematician from attacking the problem. Again in 1837, Mr. Airy repeats the same idea: "If these errors are the effect of any unseen body, *it will be nearly impossible ever to find out its place.*"

The first serious attempt to discover the place of this disturbing body, was made by a young man (Mr. J. C. Adams, of Cambridge University) in England. It should be remembered that in accordance with the Newtonian law of gravitation, every body in the solar system attracts every other; that the attraction of each body is proportioned to its quantity of matter; and that in the same body the power of attraction varies inversely as the square of the distance. In order therefore to compute the exact place of a planet, in its orbit about the sun, it is necessary not merely to regard the attraction of the central body, but also to allow for the influence of all the other bodies of the solar system.

Hitherto mathematicians had only aspired to compute the disturbing influence of one body upon another, when the magnitude and position of both bodies were known. But in the case of Uranus, it was required to solve the inverse problem which Professor Airy had pronounced hopeless, viz., from the observed disturbances of one body, to compute the place of the disturbing body.

After taking his degree of Bachelor of Arts in January, 1843, with the honor of Senior Wrangler, Mr. Adams ventured to attack this problem, and obtained an approximate solution, by sup-

posing the disturbing body to move in a circle, at twice the distance of Uranus from the sun. His results were so far satisfactory, as to encourage him to attempt a more complete solution. Accordingly in February, 1844, having obtained through Professor Airy, a complete copy of the Greenwich observations of Uranus, he renewed his computations, which he continued during that and the subsequent years. In September, 1845, he had obtained the approximate orbit of the disturbing planet, which he showed to Professor Challis, the director of the Observatory at Cambridge; and near the close of the next month, he communicated his results to the Astronomer Royal, together with a comparison of his theory with the observations. The discrepancies were quite small, except for the single observation of 1690. Professor Airy, in acknowledging the receipt of this letter, pronounced the results extremely satisfactory, and enquired of Mr. Adams whether his theory would explain the error of the tables in regard to the distance of Uranus from the sun, which error he had shewn to be very great. To this enquiry Mr. Adams returned no answer for nearly a year; probably because he was not able to answer the question entirely to his own satisfaction.

Meanwhile this grand problem was undertaken by another mathematician, who was entirely ignorant of the progress which Mr. Adams had made; for none of his results had yet been published. In the summer of 1845, M. Arago of Paris, requested M. Le Verrier, a young mathematician who had already distinguished himself by his improved tables of Mercury, to attempt the solution of this problem. This he accordingly did, and his success astonished all Europe. He commenced his investigations by enquiring whether the observations of Uranus could be reconciled with the supposition, that this body is subject to no other attraction than that of the sun and the known planets, acting according to the Newtonian law of gravitation. He carefully computed the effects due to the action of Jupiter and Saturn, neglecting no quantities until he had proved that their influence was insensible. He thus discovered some important terms which had been neglected by Laplace. He then compared his theory with observation, and proved conclusively that the observations of Uranus could not be reconciled with the law of gravitation, except by admitting some extraneous action. These results were communicated to the Academy of Sciences, Nov. 10, 1845; and such was the reputation secured by this and his preceding memoirs, that in January, 1846, he was elected to fill the vacancy which had occurred in the Institute, in the section of Astronomy, by the death of Cassini. This memoir was but preliminary to his grand investigation; and it should be remarked, that Mr. Adams *had already deposited* with the Astronomer Royal at Greenwich, a paper containing the elements of the supposed disturbing planet,

and agreeing closely with the results which Le Verrier subsequently obtained.

Le Verrier next proceeds to enquire after the cause of the discovered irregularities. Is it possible that at the immense distance of Uranus from the sun, the force of attraction does not vary inversely as the square of the distance? The law of gravitation is too firmly established to permit such a supposition, until every other resource has failed. Are these irregularities due to the resistance of a rare ether diffused every where through space? No other planet has afforded any indications of such a resistance. Can they be ascribed to a great satellite accompanying the planet? Such a cause would produce inequalities having a very short period; while the observed anomalies of Uranus are precisely the reverse. Has a comet impinged upon Uranus, and changed the form of its orbit? Such a cause might render it impossible to represent the entire series of observations by a single elliptic orbit; but the observations *before* the supposed collision would all be consistent with each other, and the observations *after* collision would also be consistent with each other. Yet the observations of Uranus from 1781 to 1821, can neither be reconciled with the earlier observations nor with the more recent ones.

There seems to remain no other probable supposition than that of an undiscovered planet. But if these disturbances are due to such a body, we cannot suppose it situated within the orbit of Saturn. This would disturb the orbit of Saturn more than that of Uranus, while we know that its influence on Saturn is inappreciable, for Saturn's motion is well represented by the tables. Can this body be situated between Saturn and Uranus? We must then place it much nearer Uranus than Saturn, for the reason already assigned, in which case its mass must be supposed to be small, or it would produce too great an effect upon Uranus. Under these circumstances, its action would only be appreciable when in the immediate neighborhood of Uranus, which supposition does not accord well with the observations. The disturbing body must then be situated beyond Uranus, and at a considerable distance from it, for reasons already given. Now the distance of each of the more remote planets from the sun, is about double that of the preceding one. It is natural then to conjecture that the disturbing planet may be at a distance from the sun double that of Uranus; and it must move nearly in the ecliptic, because the observed inequalities of Uranus are chiefly in the direction of the ecliptic. Le Verrier then propounds the following specific problem:

"*Are the irregularities in the motion of Uranus due to the action of a planet situated in the ecliptic, at a distance from the sun double that of Uranus? If so, what is its present place, its mass, and the elements of its orbit?*" This problem he proceeds to resolve.

If we could determine for each day the precise effect produced by the unknown body, we could deduce from it the *direction* in which Uranus is drawn; that is, we should know the direction of the disturbing body. But the problem is far from being thus simple. The amount of the disturbance cannot be deduced directly from the observations, unless we know the exact orbit which Uranus would describe provided it were free from this disturbing action; and this orbit in turn cannot be computed, unless we know the amount of the disturbances. Le Verrier therefore computes for every nine degrees of the entire circumference, the effect which would be produced by supposing a planet situated in different parts of the ecliptic. He finds that when he locates the supposed disturbing planet in one part of the ecliptic, the discrepancies between the observed and computed effects are enormous. By varying the place of the planet, the discrepancies become smaller, until at a certain point they nearly disappear. Hence he concludes that there is but one point of the ecliptic where the planet can be placed, so as to satisfy the observations of Uranus. Having thus determined its approximate place, he proceeds to compute more rigorously its effects; and on the first of June, 1846, he announces as the result of his investigations, that the longitude of the disturbing planet for the beginning of 1847, must be about 325°.

The result thus obtained by Le Verrier, differed but one degree from that communicated by Mr. Adams to Professor Airy, more than seven months previous. Upon receiving this intelligence, Professor Airy expressed himself satisfied with regard to the general accuracy of both computations, and immediately wrote to Le Verrier, enquiring as he had done before of Mr. Adams, whether his theory explained the error of the tables in respect to the distance of Uranus from the sun. Le Verrier showed that it did this perfectly. Professor Airy was now so well convinced of the existence of a planet yet undiscovered, that he was anxious to have a systematic search for it forthwith undertaken. The Observatory of Cambridge is provided with one of the finest telescopes of Europe, presented by the late Duke of Northumberland. Professor Airy urged upon the director, Professor Challis, to undertake the desired search, and recommended the examination of a belt of the heavens, ten degrees in breadth, and extending thirty degrees in the direction of the ecliptic. This belt was to be swept over at least three times. If any star in the second sweep had a different position from that observed in the first, it might be presumed that it was the planet. If two sweeps failed of detecting the planet, it might be caught in the third.

Professor Challis commenced his search, July 29th, and continued it each favorable evening, recording the exact position of every star down to the eleventh magnitude. Meanwhile Le Ver-

rier was proceeding with his computations, and on the 31st of August, he announced to the Academy the elements he had obtained for the supposed planet. He assigned its exact place in the heavens, and estimated that it should appear as a star of the eighth magnitude, with an apparent diameter of about three seconds; and consequently that the planet ought to be visible in good telescopes, and with a perceptible disc.

Soon after this communication was made to the Academy, Le Verrier wrote to Dr. Galle of the Berlin Observatory, (where is found one of the largest telescopes of Europe,) requesting him to undertake a search for his computed planet, and assigning its supposed place in the heavens. The Berlin Academy had just published a chart of this part of the heavens, showing the exact place of every star down to the tenth magnitude. On the evening of the very day upon which this letter was received, (September 23,) Galle found near the place computed by Le Verrier, a star of the eighth magnitude not contained on the Berlin charts. Its place was carefully measured; and the observations being repeated on the succeeding evening, showed a motion of more than a minute of space. The new star was found in longitude 325° 52′; the place of the planet computed by Le Verrier was 324° 58′: so that this body was within one degree of the computed point. Its diameter measured nearly three seconds. A coincidence so exact left no doubt that this was really the body whose effects had been detected in the motions of Uranus. The news of the discovery spread rapidly over Europe. The planet was observed at Gottingen on the 27th of September, at Altona and Hamburgh on the 28th; and at London on the 30th.

We must now return to Professor Challis, whom we left exploring a large zone of the heavens, and recording the exact position of every star down to the eleventh magnitude. These observations were continued from the 29th of July to the 29th of September, during which time he had made more than three thousand observations of stars. On the 29th of September, Professor Challis saw for the first time, Le Verrier's memoir communicated to the Academy, August 31st. Struck with the confidence which Le Verrier manifested in his own conclusions, Professor Challis immediately changed his mode of observation, and endeavored to distinguish the planet from the fixed stars by means of its disc. On the same evening he swept over the zone marked out by Le Verrier, paying particular attention to the physical appearance of the brighter stars. Out of three hundred stars whose positions were recorded that night, he selected one which appeared to have a disc, and which proved to be the planet. On the first of October he heard of the discovery at Berlin; and now on comparing his numerous observations, he finds that he had *twice* observed the planet before, viz., on August fourth and twelfth; but he lost

the opportunity of being first to announce the discovery, by deferring too long the discussion of his observations.

The news of this capital discovery was brought to this country by the steamer of October 4th, and every telescope was immediately turned upon the planet. It was observed at Cambridge by Mr. Bond, Oct. 21st; it was seen at Washington, Oct. 23d, and was regularly observed there till Jan. 27th, when it approached too near the sun to be longer followed.

Le Verrier, although quite a young man, has thus established at once an enviable reputation. He has been literally overwhelmed with honors received from the sovereigns and academies of Europe. He has been created an officer of the Legion of Honor by the King of France, and a special chair of Celestial Mechanics has been established for him at the Faculty of Sciences. From the King of Denmark he has received the title of Commander of the Royal Order of Dannebroga; and the Royal Society of London conferred on him the Copley Medal. The Academy of St. Petersburg resolved to offer him the first vacancy in their body; and the Royal Society of Gottingen elected him to the rank of Foreign Associate.

Thus have the predictions of Adams and Le Verrier, with regard to the place of the planet at the present time, been wonderfully fulfilled; but is this body pursuing the orbit which these mathematicians had prescribed for it? Since its first discovery, the planet has advanced but three or four degrees in its orbit. We have less than two years' observations to determine an orbit which it requires more than a century to complete. The computation has been made; but a result derived from so short a period must be received with some distrust, on account of the unavoidable imperfection of all observations. The best observations are liable to small errors; and a slight error in the measurement of a minute portion of the orbit, would lead to a much larger error in the computed length of the remainder of the path. We must have observations for a long series of years, to furnish the orbit with all desirable precision. Under these circumstances, it becomes a question of the highest interest, whether this body may not have been observed by astronomers of former years, and mistaken for a fixed star. If we could obtain one good observation, made sometime in the last century, it would enable us at once to determine the orbit with nearly the same precision as that of Jupiter itself. It will then be presumed that astronomers have not neglected to explore the records of the past, to discover if possible some chance observation of the new planet.

Mr. Hind of London, adopting the predicted elements of Le Verrier, examined Lalande's and other observations for this purpose, and satisfied himself that the new planet was not there to be found. An American Astronomer, Mr. Sears C. Walker, was

more fortunate. Mr. Walker proceeded in the following manner. He first computed the orbit which best represented all the observations which had been made at the Washington Observatory, as well as those which had been received from Europe. He then computed the planet's probable place for a long series of preceding years, and sought among the records of astronomers, for observations of stars in the neighborhood of the computed path. Bradley, Mayer and Lacaille, have left us an immense collection of observations, yet they seldom recorded stars so small as the body in question. Among the observations of Piazzi, so far as they have been received in this country, no one was found which could be identified with the planet. The Madras observations were generally confined to the stars of Piazzi's catalogue. The Paramatta catalogue seldom extends north of the thirty-third parallel of south declination; and Bessel, in preparing his zones of 75,000 stars, did not sweep far enough south to comprehend the planet. The only remaining chance of finding an observation of the planet, was among the observations of Lalande. The Histoire Celeste Française embraces 50,000 stars, and Mr. Walker soon found that Lalande had swept over the supposed path of the planet on the 8th and 10th of May, 1795. He accordingly computed more carefully the place of the planet for this period, making small variations in the elements of the orbit, so as to include the entire region within which the planet could possibly have been confined. He then selected from the Histoire Celeste, all the stars within a quarter of a degree of the computed path. These stars were *nine* in number; of which *six* had however been subsequently observed by Bessel, and of course were to be set down as *fixed stars*. But three stars remained which required special examination; and of these, one was too small to be mistaken for the planet, and a second was thought to be too far from the computed place. The remaining star was distant only two minutes from the computed place of the planet; it was of the same magnitude, and was not to be found in Bessel's observations, although this part of the heavens must have been included in the field of his telescope. This discovery was made on the 2d of February, 1847; and on the first clear subsequent evening, Feb. 4th, the great equatorial of the Washington Observatory was pointed to the heavens, and *this star was missing*. Where Lalande in 1795 saw a star of the ninth magnitude, there remained *only a blank*. The conclusion seemed almost certain, that Mr. Walker had here obtained the object of his search. He accordingly computed the path upon this supposition, and found that a single elliptic orbit would represent, with almost mathematical precision, the observation of 1795, and all the observations of 1846.

The case seemed completely made out. *But there was a weak point in the argument.* Lalande had marked his observation of the altitude of this star as *doubtful.* Could we rest the decision of a question so important upon a bad observation? How unfortunate, that among the 50,000 stars contained in this precious collection, there was *only one* which could be presumed to have been the planet, and this observation the author had marked as doubtful! Thus the question stood—astronomers were afraid to admit, and still could not reject, the conclusions of Mr. Walker. The steamer which left Boston on the first of March, carried a copy of the Boston Courier, containing the account of Mr. Walker's researches. This paper was destined for M. Le Verrier; and on the very day of its arrival, he also received a letter from Altona, dated March 21st, announcing that M. Petersen had discovered that this very star, observed by Lalande in 1795, was *now missing* from the heavens. M. Petersen's discovery was made on the 17th of March: Mr. Walker made the same discovery, theoretically, Feb. 2d; and it was confirmed by an actual inspection of the heavens, Feb. 4th. Mr. Walker then has the priority of *six weeks* in the discovery. Fortunately, the original manuscripts of Lalande had been preserved, and were deposited in the Observatory of Paris. On consulting them, it was found that the doubtful mark appended to the published observations, did not exist in the manuscript. Moreover the star had been twice observed, viz., on the 8th and 10th of May, 1795; but as the two observations did not agree, Lalande suppressed the former, and in his printed book marked the latter *doubtful.* The discrepancy between the two observations, is almost exactly that which is due to two days' motion of the planet, according to the orbit of Mr. Walker.

Thus then we have most unexpectedly secured *two good observations* in place of *one doubtful one.* We can no longer withhold our full belief. A single elliptic orbit represents with great precision, the two observations of Lalande, and all the observations of the past year. Let us then compare the predicted orbits of Adams and Le Verrier, with the true orbit according to Mr. Walker. The comparison stands as follows:

	Adams.	Le Verrier.	Walker.
Longitude of the perihelion, -	299° 11′	284° 45′	48° 21′ 3″
Longitude of ascending node,	unknown.	156 0	130 4 35
Inclination of the orbit, - -	unknown.	6 0	1 47 0
Mean longitude, Jan. 1, 1847,	323° 24′	318 47	328 31 56
True longitude, Jan. 1, 1847, -	329 57	326 32	327 33 47
Eccentricity, - - - - -	0·12061	0·10761	0·008577
Mean distance from the sun, -	37·25	36·154	30·03666
Time of revolution in years, -	227·323	217·387	164·6181
Mean daily motion, - -	15″·609	16″·318	21″·55448
Mass, - - - - - -	$\frac{1}{6666}$	$\frac{1}{9300}$	$\frac{1}{15000}$

The orbits of Adams and Le Verrier agree remarkably well with each other, but differ sadly from that of Mr. Walker; that is, we are compelled to admit that they differ materially from the truth. They represent remarkably well the *direction* in which the planet is now seen from the earth, but they give its mean distance too great by *six hundred millions of miles*. This discrepancy is so enormous as to have given occasion for the remark, that the planet actually discovered is *not the planet predicted by Le Verrier*. Certainly we must concede, that the region of space occupied by the planet is very remote from that prescribed for it by this mathematician.

But how has it happened that two astronomers have arrived, by independent computations, at almost identically the same result, when both are so seriously in error? The answer is obvious. Since it was necessary in the first instance, to make some hypothesis with regard to the distance of the disturbing body from the sun, both computers started with that supposition which was generally thought most probable. The distance of Saturn from the sun is nearly double that of Jupiter; the distance of Uranus is almost exactly double that of Saturn; hence it seemed probable that the planet they were in search of, would be found at a distance about double that of Uranus. Accordingly, this assumption was made the basis of their first computations; but neither of the computers accepted this as his final result, without attempting to verify it. They both varied the assumed distance, and found that by bringing the planet a little nearer the sun, the observed irregularities of Uranus were still better explained. The distance of 36·154 (or about 3435 millions of miles) finally adopted by Le Verrier, was that which appeared to reconcile all the observations *most satisfactorily*. This distance corresponds to a period of two hundred and seventeen years. Le Verrier found that whether he increased or diminished this distance, the observations of Uranus were not so well represented. He hence inferred that the period could not be less than two hundred and seven years, nor more than two hundred and thirty-three years. According to Professor Peirce of Harvard University, this conclusion was not a legitimate one. The equations employed by Le Verrier were computed on the supposition, that the period of revolution was *about two hundred and seventeen years*, and they were only applicable to a period *not differing greatly* from this quantity. His equations therefore did not authorize him to infer with certainty, any thing whatever with regard to orbits differing very much from the one he employed. The true period is believed to be about one hundred and sixty-four years, which is nearly double the period of Uranus. Now a planet revolving in such an orbit must exert an influence upon Uranus, which is very peculiar, and for which the analysis of Le Verrier made no pro-

vision. Although then, by a singular coincidence, the computations of Adams and Le Verrier assigned to the disturbing planet *at the present time* a *direction* in the heavens extremely near the truth, and thus fortunately led to its discovery; still the region of space which they had prescribed for it, differs enormously from the truth, and their analysis is inapplicable to the problem actually presented. Whether the planet discovered by Dr. Galle, will explain, *either wholly or in part*, the observed anomalies of Uranus, cannot be legitimately inferred from the analysis of Adams or Le Verrier. Professor Peirce then has reason for asserting, that the planet actually discovered *is not the planet to which geometrical analysis had directed the telescope;* its orbit is not contained within the limits of space which have been explored by geometers searching for the source of the disturbances of Uranus; and its discovery by Galle must be regarded as a happy accident.

Some who are very zealous for the honor of astronomy, have been unwilling to admit that *accident* had any thing to do with the discovery of this planet. Such persons will probably admit that accident had some concern with the *time* when this problem began seriously to occupy the attention of astronomers. In the fore part of the year 1845, the true orbit and that of Le Verrier agreed in assigning to the planet *exactly* the same longitude; but such a coincidence had not occurred for *more than six hundred years* before, and will not occur again in another six hundred years. If Le Verrier had obtained precisely the same elements for the disturbing planet half a century earlier or later, and astronomers had searched for it, they would not have found it. The computed orbit would have served only to mislead—it would have diverted attention to the wrong part of the heavens,—for the planet would have been nearly thirty degrees distant from the place assigned by the elements of Le Verrier. It was certainly a very singular, and a very "happy accident," that an erroneous orbit which assigned to the planet its true direction in the heavens only once in a thousand years, should have been published to the world almost at the very instant of the coincidence. It is as if one should expect to find the place of Uranus by consulting the tables of Saturn. In the year 1852, Uranus may be found by looking near the place assigned for Saturn in the Nautical Almanac, the two planets being then in conjunction; but the like will not happen again for more than forty years. Would it not then be called an accident if the tables of Saturn should inform the observer exactly where to look for Uranus?

To some it has appeared a matter of surprise, that the new planet was not sooner discovered. Le Verrier's second memoir, which assigned the probable place of the disturbing body, was presented to the Academy on the 1st of June; and his third me-

moir (containing every thing which Dr. Galle had in his possession at the time of his discovery) was presented August 31st; yet Galle's discovery was not made till Sept. 23d. What were the astronomers of Paris doing meanwhile? Why did they not immediately point their telescopes to the heavens? Why did they neglect the opportunity of securing to France the glory of both the theoretical and practical discovery, and leave to a German astronomer the verification of the sublimest theory of modern science? The answer is plain. *The astronomers of Paris did not expect to find a planet within one degree of the place computed by Le Verrier.* Le Verrier himself did not expect it. He assigned the *most probable* place of his planet in longitude 325°. He expressed the opinion that its longitude would not be *less* than 321°, nor *more* than 335°. But he adds, "If the planet should *not* be discovered within these limits, then we must extend our search beyond them (*on recourrait aux longitudes superieures.*") If he was sure of being able to find his planet without a long continued and laborious search, why did he not borrow a telescope, and at once verify his own predictions?

Nor had the astronomers of the rest of Europe much higher faith than those of Paris. Professor Encke in announcing the discovery, characterizes it as "*far exceeding any expectations which could have been previously entertained.*" That Professors Airy and Challis, although they were pretty well satisfied of the *existence* of a planet yet undiscovered, regarded its exact place in the heavens as extremely uncertain, is plain from their comprehensive plan of observation, viz., to sweep three times over a belt of the heavens, 30 degrees in length, and 10 degrees in breadth; a plan which Professor Challis states it would have been impossible for him to complete within the year 1846.

Do we then charge Encke and Airy with a want of sagacity? By no means. On the contrary, we maintain that *they had no reason to expect to find the planet within one degree of the computed place.* Le Verrier's own statement of the limits within which the planet should be sought for, is sufficient proof of this. But we go further. Le Verrier thought his problem was capable of but one solution; that is, that there was only one point of the heavens in which the disturbing planet could be placed so as to account for the motions of Uranus. In this (according to Professor Peirce's computations) he was mistaken. Professor Peirce announced in April, 1847, that he had discovered two other solutions, which are decidedly different from each other, and from that of Le Verrier, and equally complete with his. Moreover, Le Verrier ascribed the whole effect in question to *one* planet; while it is *almost certain* (we are half inclined to omit the *almost*) that more than one body is concerned in producing the effect. Professor Challis therefore proceeded like a sagacious, as well as brave gen-

eral. He contemplated a long siege—yet his plan rendered ultimate success almost certain. Dr. Galle took the citadel by storm —yet his success must have astonished himself as much as it did the world. He had no reason to expect so easy a conquest.

Some difficulty at first occurred in deciding upon a name for the new planet. The Bureau des Longitudes of Paris were in favor of calling it *Neptune*, and this name was given out by Le Verrier in private letters to different astronomers in England and Germany. Subsequently, Le Verrier commissioned his friend Arago to give the planet a name; and Arago declared he would never call it by any other name than *Le Verrier*. When Sir William Herschel discovered a planet, he named it Georgium Sidus; and the name of "the Georgian" is still retained in the English Nautical Almanac. But this name being offensive to the national pride of the French, they at first called the planet Herschel, and afterwards Uranus. The latter name has come into exclusive use on the continent; but Arago, in order to secure an honor to his friend Le Verrier, proposed to restore the name of Herschel, and also that each of the smaller planets should receive the name of its discoverer.

The astronomers of Europe refused to concur in the decision of Arago. There are objections to this principle of nomenclature, some of which have considerable weight. The name of the discoverer of a planet may happen to be immoderately long, or ludicrously short; difficult to pronounce, or comically significant. Then also, if the same astronomer should be fortunate enough to discover more than one planet, we should be obliged to repeat the surname with a prefix. Already we have two planets discovered by Olbers; two discovered by Hencke, and two by Hind.

Moreover it often happens that several persons contribute an important part in the discovery of the same body. Thus the planet Ceres was first discovered by Piazzi in the course of a series of observations having a different object in view. After a few weeks, the planet became invisible from its proximity to the sun. Astronomers computed the orbit from Piazzi's observations, and searched for it some months afterwards, when it ought again to have come into view. But the planet could not be found. Ceres was entirely lost, and would not have been seen again, had not Gauss, by methods of his own invention, computed a much more accurate orbit, which disclosed the exact place of the fugitive, and enabled De Zach to find it immediately upon pointing his telescope to the heavens. To Gauss, therefore, belongs the honor of being the *second discoverer* of Ceres; and the second discovery was far more glorious than the first.

The discovery of the new planet has been justly characterized by Professor Airy, as "*the effect of a movement of the age.*"

The honor of the discovery is not to be exclusively engrossed by either Adams or Le Verrier. The labors of numerous astronomers had prepared the way, and contributed more or less directly to the discovery. An eminent critic ridicules this idea. But Mr. Adams himself informs us, that his attention was first directed to the subject of the motions of Uranus, by reading Airy's report on the recent progress of astronomy; and Le Verrier states, that in the summer of 1845, he suspended the researches on comets, upon which he was then employed, to devote his time to Uranus, *at the urgent solicitation of M. Arago.* Omitting several who have indirectly contributed to this discovery, we may mention five whose names will ever be honorably associated with the planet Neptune; viz., Adams, Challis, Le Verrier, Galle and Walker. Adams first determined the place of the new planet from the perturbations of Uranus. Professor Challis was the first to institute a systematic search for the planet, and had actually secured two observations of it, before it was seen at Berlin. True, he did not at the time know that he had found the planet, for he had not interrogated his observations. But the prize was secured, and he would infallibly have recognized it, as soon as he had instituted a comparison of his observations. In his eager zeal to make sure of the diamond, he shoveled up with it a great mass of rubbish, and stored it all away to examine at his leisure.

To Le Verrier belongs the credit of having been the first to publish to the world the process by which he arrived at the conclusion of the existence of a new planet; and it is conceded that his researches were more complete and elaborate than those of his rival. To Galle belongs the undisputed honor of having been the first practically to recognize this body as a planet; and to our own countryman belongs the glory of having traced this body backward in his journeyings for more than half a century, and out of 50,000 stars recorded by Lalande, singled out one as being identical with the planet Neptune.

To give to the new planet the name of Le Verrier, would be indeed to confer honor where honor was due; but it would be dishonor to others, whose pretensions are but little inferior to his own. The astronomers of Europe have preferred to take a name from the divinities of the Greek mythology, in conformity with a well established usage; and as the name of Neptune harmonizes with this system, and withal was first suggested by the Bureau des Longitudes, they are disposed to adhere to it. This is the decision of the astronomers of Great Britain, of Germany, of Russia, and of Europe generally, with the exception perhaps of France; and indeed we infer that Arago himself has yielded to the force of the general current. The Comptes Rendus of the Academy, for August, 1847, notices the planet *of* M. Le Verrier; and the Comptes Rendus for September, speaks of *the planet Neptune.*

The discovery of five new planets within the period of a few months, suggests the probability that astronomers may ere long be forced to adopt some new system of nomenclature. There seems good reason to believe that in a few years we shall witness a large addition to the members of the solar system, and it will require a strong memory to retain the names of all of them. The present names are entirely arbitrary, and are not at all significant. Indeed their only recommendation is that they suggest no ideas, and therefore can give offense to no one. But if the number of the primary planets should increase to fifty, this system (or rather want of system) would become intolerable. Some new nomenclature would then become indispensable.

The discovery of Neptune has given an unequivocal refutation to Bode's law of the planetary distances. This famous law may be thus stated. If we set down the number 4 several times in a row, and to the second 4 add 3, to the third four add twice 3 or 6, to the next 4 add twice 6 or 12, and so on as in the following table, the resulting numbers will represent nearly the relative distances of the planets from the sun.

4	4	4	4	4	4	etc.
	3	6	12	24	48	etc.
4	7	10	16	28	52	etc.

If the distance of the earth from the sun be called 10, then 4 will represent *nearly* the distance of Mercury; 7 that of Venus; and so of the rest. This law was never accurately verified in the case of any of the planets, and Neptune forms a decided exception to it. In order to exhibit this fact more clearly, we have prepared the following table, shewing first the true relative distance of each of the planets; secondly, the distance according to Bode's law; and thirdly, the error of this law.

	True dist	Bode.	Error		True dist.	Bode.	Error.
Mercury,	3·87	4	0·13	Jupiter,	52·03	52	0·03
Venus,	7·23	7	0·23	Saturn,	95·30	100	4·61
Earth,	10·00	10		Uranus,	191·82	196	4·18
Mars,	15·24	16	0·76	Neptune,	300·37	388	87·63
8 Asteroids,	25·54	28	2·46				

It will be seen from this table, that although this law represents pretty well the distance of the nearer planets, the error is quite large for Saturn and Uranus, and for Neptune the error is altogether overwhelming, amounting to more than *eight hundred* millions of miles, a quantity almost equal to the distance of Saturn from the sun. It is mere mockery to honor such coincidences with the name of a *law*. A law of nature is *precise*—it is capable of exact numerical application. Let then the preceding rule be called the law of Bode; ***it is not a law of nature.***

It is the opinion of some observers, that Neptune is surrounded by a ring like Saturn. Mr. Lassell of Liverpool, has an excellent Newtonian reflector of twenty feet focal length and two feet aperture, with which he has made numerous observations of the planet. On the 3d of October, 1846, he was struck with the shape of the planet, as being not that of a round ball; and again on the 10th of October, he received a distinct impression that the planet was surrounded by an obliquely situated ring. On the 10th of November, the planet appeared very much like Saturn, as seen with a small telescope, and low power, though much fainter. Several other persons also saw the supposed ring, and all in the same direction. During the past season, Mr. Lassell frequently saw the same appearance again, and found its angle of position to be 70 degrees S. W. He also satisfied himself that this appearance did not arise from any defect in his telescope.

Professor Challis states that on the 12th of January, 1847, he received for the first time a distinct impression that the planet was surrounded by a ring. Two independent drawings made by himself and his assistant, gave the annexed representation of its appearance. On the 14th he saw the ring again, and was surprised that he had not noticed it before. The ratio of the diameter of the ring to that of the planet, was about that of three to two.

On the other hand, the great telescope at Cambridge, Mass., exhibits *no ring*. The following is an extract of a letter from the director, Mr. W. C. Bond. "In regard to Neptune we have never been able to satisfy ourselves of the existence of a ring. On the contrary, both my son George and myself have repeatedly had opportunities of examining Neptune under high powers with the full aperture of fifteen inches, and have seen only a round disc, while our micrometrical measures of his diameter agreed well together. *The only times when we have observed any thing like projections, have been during unfavorable states of the atmosphere, and at low altitudes.*"

When we consider that Prof. Challis saw the supposed ring while the planet was very near the horizon, we may easily persuade ourselves that the elongation observed was an atmospheric phenomenon. It seems more difficult to explain away Mr. Lassell's observations, as they were made with an excellent instrument, and have been many times repeated. Nevertheless, when we remember the difference of latitude of the observers, we cannot help feeling that Mr. Bond's negative testimony is as good as Mr. Lassell's positive testimony, and if we do not entirely deny the possible existence of a ring, we must at least hold our minds in suspense, and wait patiently for further evidence. It is possible that this question may never be fully cleared up, until some more powerful telescope is turned upon the planet, or it can be observed in a different part of its orbit.

It is considered certain that Neptune is attended by a *satellite.* Mr. Lassell states that on the 10th of October, 1846, he observed a faint star distant from the planet about three diameters. On the 11th and 30th of November and also Dec. 3d, he saw a small star having about the same appearance; and he considered it probable that the star was a satellite. On the 7th of July, 1847, he again saw the supposed satellite, and on the following evening, the planet and satellite had both changed their position with reference to the neighboring stars. On the 22d, 25th and 26th of the same month, the planet appeared attended by a satellite, and on the 1st of August he obtained the fullest evidence of the verity of the satellite, in being able clearly to ascertain that during the two hours he watched the planet, it had carried the satellite along with it in its orbital motion. On the 20th of September last, Mr. Lassell announced that he had obtained twenty observations of the satellite during the current year, and from them all he had concluded that the period of the satellite was five days, twenty hours, fifty minutes, and forty-five seconds; and he thinks this result cannot be in error beyond a small number of minutes. The radius of its orbit is about eighteen seconds, which gives about 250,000 miles for the distance of the satellite from the planet. The satellite is always more brilliant in the S. W. than in the N. E. part of its orbit, presenting in this respect a striking analogy with the outer satellite of Saturn. In the former position Mr. Lassell found it easy to observe; in the latter extremely difficult.

The conclusions of Mr. Lassell have been confirmed by the observations of Mr. Bond at Cambridge. Mr. Bond believes that he saw the satellite in August last, and in October he obtained satisfactory measures of its distance and angle of position by means of a micrometer with illuminated wires.

The preceding results enable us to compute the mass of Neptune, which is found to be about $\frac{1}{19360}$ part of the sun.

Mr. Bond states that he has at times been quite confident of seeing a *second* satellite, but has never yet been able to obtain successive measures of its distance from the primary.

The grand question still remains untouched, *Will the new planet explain the observed irregularities in the motion of Uranus?*

The planet having been actually discovered in the heavens by means of certain predicted elements, and within one degree of the predicted place, the natural conclusion was that those elements were extremely near the truth, and that the planet would perfectly explain those effects by whose study its own existence had been detected. When, however, observation had rendered it certain that the planet moved in a much smaller and less eccentric orbit than had been predicted, if became doubtful whether it would account for the anomalies in the motion of Uranus. When

Le Verrier, in March, 1847, received notice of the computations of Mr. Walker, who obtained an orbit differing but little from a circle, he at once pronounced the small eccentricity *incompatible* with the observed perturbations. Mr. Adams, in a letter of June 11, 1847, says "I am hard at work on the perturbations of Uranus, in order to obtain a new theoretical determination of the place. The general values of the perturbations are enormous, far exceeding any thing else of the same kind in the system of the primary planets. A comparison of the numerical expressions, for the perturbations which I have now obtained, with those which I used before, would *justify some skepticism as to former conclusions*. But we shall soon see how this great apparent difference affects the result."

By referring to the table on page 195, it will be seen that Adams and Le Verrier explain the anomalies of Uranus, by assuming a very large body (having twice the mass of Uranus) moving in a very eccentric orbit. It is now discovered that the orbit hardly differs at all from a circle, and that the mass is only about one half of that which had been assumed. Now the more eccentric the orbit, the greater must be the *inequality* of a planet's action upon other bodies; that is, the greater the perturbations it must cause in their motions. One half of the observed irregularity in the motion of Uranus, was explained by Adams and Le Verrier by means of the great eccentricity ascribed to their hypothetical planet. This half is now gone with the failure of the eccentricity. But on the other hand, Neptune is found to be much *nearer* Uranus than the hypothetical planet, and in consequence of its proximity its disturbing action is *increased*, so that these two variations of the elements in a measure compensate each other.

But it is also discovered that the mass of Neptune is materially less than the hypothetical planet of Le Verrier, and on this account its disturbing action is diminished. To what extent the planet Neptune will account for the perturbations of Uranus can only be determined by a rigorous and careful computation. But this computation involves no new theoretical difficulty. We now know the mass of Neptune and its orbit; and we can employ the same methods which have long been practiced in the case of the other planets. The methods of computation are well known, but the operation is laborious. Messrs. Peirce and Adams are now occupied with this problem, and it is presumed Le Verrier also; but the results of their labors are yet only partially known. It is understood that Prof. Peirce has found it impossible to explain the whole perturbation of Uranus by the action of Neptune.

On the whole then we seem authorized to conclude, that the planet Neptune is not the hypothetical planet of Le Verrier, and for the following reasons:

1. The orbit of Le Verrier's planet was highly eccentric, while that of Neptune is almost circular.

2. The mean distance of Neptune from the sun is six hundred millions of miles less than that of the planet of Le Verrier.

3. The mass of Neptune is only about one half that of the planet of Le Verrier.

4. The hypothetical planet of Le Verrier perfectly explained the inequalities of Uranus, while Neptune explains them only partially.

If then Neptune is not the hypothetical planet of Le Verrier, some may conclude that the hypothetical planet remains to be discovered. Such a conclusion is entirely erroneous. The hypothetical planet of Le Verrier *does not exist.* The state of the question is this. A certain *effect* was observed in the motions of Uranus. Le Verrier proved by legitimate reasoning that if we admit the existence of a certain planet, this effect is fully explained. This hypothesis led to the discovery of Neptune. It is now ascertained that Neptune will not explain the *whole effect* observed; but it will explain *a part* of it. We cannot then admit that Le Verrier's hypothetical planet is still to be discovered, because its existence would involve a greater effect than that which *now remains* to be explained. But the conclusion seems irresistibly forced upon us that there does exist a planet yet undiscovered, to which we are to ascribe that portion of the perturbations of Uranus which remains unexplained. This unexplained inequality now demands the united attention of mathematicians. Let them deduce from the observations the most probable elements of the hitherto undiscovered planet, and then let the great telescopes of American and European observatories be put in requisition to ferret out this unseen troubler of the planetary motions.

ART. XXIII.—*Notes upon the Drift and Alluvium of Ohio and the West;* by CHARLES WHITTLESEY, of Cleveland, Ohio.

IN the following pages I have endeavored to give some facts, unincumbered by theory, relating to the superficial deposits of Ohio in particular, and of some other parts of the West, which I have observed. The names used to designate different beds or strata, will be provisional and common.

In *Ohio*, beginning at the stratified rocks, the upward section is as follows:

1st. "*Blue* hard pan," resting unconformably on the surface of the stratified rocks.

2d. "*Yellow* hard pan," resting unconformably on the stratified rocks, and the "blue hard pan."

3d. "Sand and gravel drift," containing granite bowlders (in small numbers) of large size, and unconformable to Nos. 1, 2, and the other rocks.

4th. The "valley drift," composed principally of debris of the adjacent rocks, and occupying the lower parts of the great valleys of drainage.

5th. "Lacustrine deposits," occupying the basin of the lakes, and for Lake Erie divided into

(1st.) The "blue marly sand."

(2d.) Coarse sand and gravel.

6th. Boulders.

7th. Alluvium.

Of course these beds or strata are not found in every part of the country, and all superimposed on each other. In places, and over much the largest area, the "blue hard pan," being the lowest, covers the indurated rocks.

Sometimes the "yellow hard pan," *overlapping* the blue, extends beyond it, and where the blue is wanting, rests on the subjacent rocks. So with the next oldest bed, the "sand and gravel drift," which forms the surface and soil in a large portion of the state. The blue and yellow hard pans thinning out and disappearing, the drift beds fill the place of the lowest stratum, and come in contact with the older rock formations. In the great wheat counties of Columbiana, Stark, Wayne and Richland, this member is very thick, and furnishes a loose, warm, easily tilled soil, peculiarly adapted to that grain.

The *thickness* of these three beds is very variable, seldom exceeding one hundred feet, and the yellow is commonly *passed through*, where it is the surface bed, in digging wells for water. Their thickness is as great in the highest portions of the state, elevated from 600 to 800 feet above Lake Erie, or 1150 to 1350 feet above the ocean, as elsewhere.

No. 1.—Bed No. 1 is a very compact mass of *blue clay*, *marl* and *sand*, including great numbers of small, partially water worn, *crushed* and *striated pebbles*, principally fragments of blue limestone and primitive rocks. It has also specimens of the Silurian and secondary rocks. It contains lime, so much as to effervesce with acids, and to hasten vegetation when applied to land. Besides its strong *blue color*, it is characterized by *imbedded timber*, dirt beds, leaves, sticks, and what are called by well diggers, "grape vines."

It is so solid, as to be almost impervious to water, and is very difficult to excavate. Water is frequently struck near its surface, after penetrating the overlying yellow hard pan.

The pebbles are only *partially* rounded, the angles being blunted and worn, but the stone not worked down to a spherical or elliptical form, indicating less movement and transportation than

member No. 3. They seldom have a diameter of *six* inches. The buried timber and other relics will be noticed hereafter.

No. II.—This is also a compact material, of a dull *yellow* color, with fewer stony fragments or pebbles, and less calcareous and more aluminous matter than No. I. It is not quite as solid as the blue, more pervious to water, and contains more and larger pieces of primitive rocks. I have however seen but few granite, gneiss, or gneissoid fragments, with a diameter of one foot; although I have seen *quartz* and quartzose boulders of a greater size. The clays of the country, used for bricks, are principally of this bed.

It occupies much more of the *surface* than the blue, but less than No. 3; forming a hard, stiff soil, adapted for grass. The flat regions and savannas of the northwest quarter of the state, are caused by the surface presence of this bed.

No. III.—What I have designated as the "sand and gravel drift," exhibits little regularity of stratification. It is composed of inferior patches of coarse sand and gravel, intermingled at all inclinations, evidently the result of long continued and vigorous action of water in rapid motion. The gravel is coarse, but much *worn*, *rounded* and *smooth*, like the gravel beds of rapid streams. The portion of earthy matter is about one half, of a reddish and yellowish color, showing the presence of oxyd of iron, and containing various proportions of sand and clay. Almost every rock in the northern part of America is represented in the gravel; but the greatest part by far is from the underlying and adjacent strata. There are pebbles of quartz, trap, granite, gneiss, conglomerate, limestones of all ages, iron ore, slate, coal and sandstone. In this there has been found timber but very rarely.

No. IV. *The "valley drift."*—In external appearance, the gravelly deposits of the *valleys* of the principal streams, resemble the member, No. 3, just noticed; but they evidently belong to a subsequent period, to the era of the retiring of the waters. It is more gravelly, and less earthy, and the gravel is more of local origin. For instance, in the valley of the Great Miami, especially in the lower part, as in Whitewater township, Hamilton county, Ohio, there is a thickness of about eighty feet of pebbles, almost all from the limestone of the region. And it is much the same in the valley of the Scioto, which passes on and along the eastern edge of the cliff limestone. On the Muskingum, a stream entirely within the coal series, there is a variety in the pebbles, corresponding to the rocks of that series.

Beds of sand are less common in this, than in No. 3; and especially at the edges where it overlies, or abuts on the *older drift*, there is much mixture and confusion.

It rises seldom more than fifty feet above the present level of the streams, unless at or near their mouths, or along the Ohio, where it is sometimes one hundred feet thick. The gravel of the

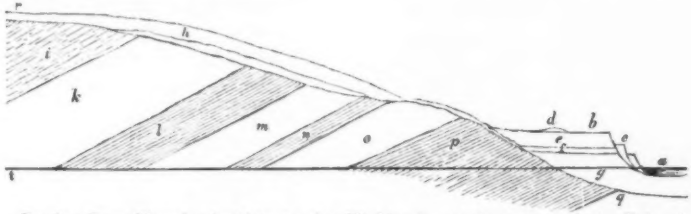
Ohio River drift, contains all the varieties of rocks known in North America. The city of Cincinnati stands upon a plain of this sort, elevated in benches or steps—the first about 60, the second 90 to 120 feet above low water.

It is sometimes difficult to distinguish this drift from the ordinary recent deposits or alluvium of the streams. On its surface are seen longitudinal ridges like those around the lakes, but less regular, and much less continuous. They are narrower, not as level, steeper at the sides, and short.

No. V. *Deposits of the Lake Basin.*—Mr. Featherstonehaugh gave these deposits the name of "lacustrine," which is probably correct. As yet however I am not aware of any well authenticated case of *fossil shells* found *in* them, though there are some of shells found in alluvium *upon* them. The descriptions given by the New York geologists, of the tertiary clays of the Hudson and of Lake Champlain, correspond closely with the external characters of the two members of the Lake Erie deposits. The "Post-pliocene tertiary" of New York, is *first*, stiff blue clay; *second*, yellowish brown clay; *third*, yellowish brown sand. The greatest height above the ocean, in the Champlain tertiary, is about 400 feet, or 164 feet below the present level of Lake Erie. In New York, *marine* shells are abundant; here neither fresh nor salt-water shells have been found. On the St. Lawrence, marine shells are numerous in the drift beds near Montreal, at an elevation of 540 feet above tide, but still below the present level of Lake Erie. In the diluvial clays and drift of New York and the St. Lawrence, *primitive boulders* of large size appear to be common; while here, none of large size are seen, so far as I am informed, and *few* of a small size.

The blue "marly sand," commonly called the "blue clay" of Lake Erie, is seen skirting the shore almost every where, if the coast is not rocky,—its upper face nearly horizontal, and rising from forty-five to sixty feet above the water. It is of a light blue color, so fine as scarcely to show between the fingers any grit, homogeneous, and in a dry state compact, but brittle. Very rarely, may be seen a primitive pebble, thin layers of leaves, and lignite. It is distinctly and horizontally laminated, and at Cleveland is composed of about 75 per cent. impalpable sand, 3 per cent. iron, 6 to 7 per cent. carbonate of lime, 9 per cent. carbonate of magnesia, and of vegetable matter and sulphur. It is impervious to water, and thus causes thousands of springs to appear at its surface, which passing out over the edges, dissolve and carry it away very fast, forming a quicksand. Its edge is presented to the action of the waves, which dissolve and carry it away rapidly. As it is not tenacious like clay, and not capable of sustaining itself under its own weight, and that of the sand stratum that rests upon it, there are continual breaks and slides along the

banks, on both the American and Canadian shores.—(See section.) These avalanches of earth are from *one* to *four* rods in width, breaking off in irregular patches, and sometimes sinking, in a night or in a few hours, twenty or thirty feet, leaving huge fissures through which the water of the springs passes, and rapidly washing the earth into the lake.



Section from Cleveland, Ohio, to the Highlands, southwest.—*a*, Lake Erie; *b*, Cleveland; *c*, slides; *d*, ridge; *e f g*, Lacustrine; *e*, gravel; *f*, sand; *g*, marl; *h*, superficial deposits; *i*, coal series; *k*, conglomerate; *l*, olive shales and bands of sandstone; *m*, grindstone grit; *n*, red shale; *o*, fine grained sandstone; *p*, black shale with bands of iron and sandstone; *q*, cliff limestone. Height of lake level above the ocean, 564 feet; height *rt*, 600 feet.

At the water's edge, the slide frequently raises a bank of about the width of the break, several feet above the surface, driving back for a short time the line of the shore. But the waves acting incessantly dissolve the new barrier, and soon commence their attacks upon the body of the fallen mass, which disappears, and is before long followed by a fresh avalanche from above.

At the city of Cleveland, where the bluff shore rises seventy feet above the lake, the encroachment since the survey of the town in 1796, has been at the foot of Ontario street 265 feet. The Canadian shore, from Detroit River to Long Point, is losing faster than the American. Between Port Stanley and Port Burwell on the British side, the superior face of the blue marl is about sixty feet, or fifteen feet higher than at Cleveland, and has in the upper part a lighter or more yellow color. In composition, the yellowish portion is more argillaceous than the bright blue, and appears to correspond with the yellow clay stratum of Lake Champlain.

The greatest thickness of the blue marls cannot be computed, as a large part of it lies *below* the lake level, forming the bed of more than one-half of Lake Erie. On the south shore it extends but a short distance into the interior, forming a narrow belt of low country along the lake, and thinning out as the rocks upon which it rests rise to the southward.—(See section.)

The "coarse sand and gravel" of No. V, rests conformably on No. 1 of that division, and spreads horizontally over a tract of low, and in general wet land, embracing the western half of Lake Erie, and extending westward into the states of Ohio and Michigan. On the north, it forms the soil and surface over a large por-

tion of the peninsula, between Lakes Erie and Huron; which seldom rises more than two hundred feet above the waters of these lakes. On it, and composed of its coarse water-washed sand and gravel, are seen the "lake ridges," objects of curiosity, and of much utility in a new country, being natural turnpikes that run parallel with the shore.

At Cleveland the section of this division is as follows:

1st. Grey water-washed coarse sand, resting on the blue marl —ten feet.

2d. Coarse gravel of the adjacent rocks, and sand—twenty to forty feet.

No. 1, of this section, has occasional thin bands of the "blue marl" interstratified. The wells of the place are sunk to the marl, or to one of the bands of marl in the grey sand, before water is found. The sand and gravel are so loose that the surface water sinks instantly through them, and collects at the top of the compact blue stratum, where it issues in numberless springs at an elevation of forty-five to fifty feet above the lake. In the beds of water-washed sand, above noticed, sticks, leaves, and what are called rushes, are seen at various depths, resting on what the well diggers call "sand beach."

Of the "lake ridges" I will here remark, that they are *not* precisely horizontal, and are found at *various* elevations—30, 90, 120 and 140 feet above the water. They appear to be the result of *submarine currents*, and not of the action of the waves upon a line of shore; for in that case, they should be longitudinally *level*. There are branches and cross ridges uniting different parallels, that rise and fall several feet in a mile.

Some small broken and defaced shells have been found in a ridge two miles west of Cleveland, but too much injured to decide whether they were the inhabitants of *fresh* or of *salt* water.

By reference to Mr. Stephens's notice of the superficial deposits of the western shore of Lake Michigan,* it appears that there is a similarity between the shores of that lake and of Erie.

The section at Milwaukie is at the lake level.

1st. Blue hard pan cut away by the waves and falling in slides.

2d. Beds of coarse and fine gravel irregularly stratified, and causing springs at the surface of No. 1.

3d. Fine sand without clay or pebbles.

4th. Surface deposit of reddish clay, without pebbles or boulders.

On the southern shore of Lake Superior, we find a single heavy deposit of red, marly, arenaceous clay; or more properly, a *red marly sand*, overlying the edges of sandstone conglomerate and trap rocks, and attaining a thickness of six hundred feet. It is

* Am. Jour., January, 1847.

homogeneous, horizontally stratified; will effloresce with acids; and washes into deep ravines like the Lake Erie blue marl.

There are found (but only very seldom) blocks of *trap rock*, imbedded, and boulders of native copper.

This marl and that of Lake Erie were evidently left by comparatively quiet waters. Mr. Hubbard refers the Lake Superior deposit to the tertiary.

To the southward of the trap ranges and the Ontonagan River, boulders of trap of immense size and in considerable numbers, are seen on its surface, and in the gulleys and valleys of the streams that cut through it. Its surface descends like that of the Lake Erie deposits gently towards the lake, favoring the idea in both cases, of their being the beds of an ancient and more elevated freshwater lake, descending and thinning out towards the centre.

In the vicinity of Cleveland the rate of or angle of descent, would bring down the surface of the lacustrine deposits to the surface of the waters, about two *miles* from shore; and judging from the shallowness of the lake, which at ten miles out is only about sixty feet in depth, it has gained on the land several miles since the waters assumed their present level. This is more apparent from the form of the coast, which is that of a broad bay, having rocky headlands about fifteen miles to the right and left, and ten miles advanced into the lake. The present rate of encroachment is about a mile in eight hundred years.

No. 6. *Boulders, or "erratic rocks."*—Mr. Christy of Oxford, who has travelled extensively through the west and southwest, limits the primitive boulders on the south and east, by a line from Mercer in Pennsylvania, through Zanesville, (Ohio,) Cincinnati, Princeton in Indiana, to the mouth of the Kaskaskias on the Mississippi.*

According to his observations, the materials of the diluvial beds south of the above line, are wholly of the Silurian and secondary rocks of the Western States, and primary pebbles are wanting. Proceeding southward these materials become less coarse, as might be expected from the fact of the rocks that furnish the pebbles being softer, and the distance from the supposed sources of the diluvial currents being increased. However, near the southern limits of the boulder deposit, or as it might be called, *stratum*, the blocks are of as great size and quite as abundant in Ohio as they are further north. They are here not all primitive, but in addition to the granite, trap, porphyry, gneiss and quartz rock, deposited in solitary blocks or in nests over the surface of hill and valley, we have blocks of sandstone conglomerate and cliff limestone mingled with them. Near the bend of the Cuy-

* Letter to Verneuil, 1847.

ahoga River in Summit county, Ohio, the blocks of buff colored limestone are so numerous as to be gathered and burnt for lime. There was one in the township of Bath in that county, as large as a small house, whose parent bed was evidently near the islands of Lake Erie, sixty miles northwest.

On the hills a few miles west, and also north of Fulton in Stark county, Ohio, on the Ohio canal, granitic and greenstone boulders may be seen, ten and twelve feet through, and six hundred feet above the lake. They are almost as heavy as the largest trap boulders of Lake Superior, which have been transported only a few miles from their original resting place. The inference is therefore strong, that whatever may have been the *kind* of force employed in their transportation, it continued in equal *intensity*, till they were dropped several hundred miles from the point of departure.

Primitive *boulders* are not found to my knowledge, *in the blue marl* of Lake Erie, nor *in the "blue hard pan."* I think they are not found in the "yellow hard pan," and only occasionally in the "sand and gravel drift," not more often than blocks of conglomerate Waverly sandstone, or the grits of the coal series. They have been rarely seen in the member, No. 2, of formation No. 5. They are found *upon* the lacustrine deposits, but by no means as numerous as on the upland drift. It is for this reason that I venture to call the boulder deposit here a "stratum," and the newest of all beds except the alluvium.

Our superficial deposits *differ distinctly*, in this respect, from those of the St. Lawrence, New York and New England, and also in the want of fossil shells. Mr. Lyell, Prof. Hitchcock, Dr. Jackson, and the New York geologists, agree that the boulders are there *mingled with the diluvium* or drift, at all depths, and of all sizes.

I shall soon notice another material distinction in the observed reliquiæ of the eastern and southern deposits, compared with the western, that is, in the remains of animals.

If my observations are correct, the boulder is *more recent* than the lacustrine deposits of Lake Erie as well as the diluvial beds of the uplands, lying as it does *above* and *upon* them all.

Buried Timber.—In 1844, while making a cursory survey of Hamilton county, Ohio, in reference to agriculture, I examined fifty-nine wells in different parts of the county, and of these six, or about ten per cent., had "dirt beds," leaves, timber, or "silt." They were situated at an elevation of three hundred to six hundred feet above the Ohio River at Cincinnati, which at low water is one hundred and thirty-three feet below the lake surface. As this is a subject I have not seen noticed, I shall give some details of the timber here and in other parts of the state.

Near "Carey's Academy," about seven miles north of Cincinnati, elevated four hundred or five hundred feet above the lake, and nine hundred or one thousand above the ocean, are *three* wells that show nearly the same section. They are distant from each other half a mile. The one near Mr. W. W. Carey, has the following section furnished by him.

	Feet.	Inches.
1. Surface clay and loam, - - -	18	0
2. Yellow sand, - - - - -	0	2
3. Blue marly clay, - - - -	1	0
4. Leaves, wood and sticks, - - -	0	2
5. Vegetable mould, - - - -	3	0
6. Vegetable mould and marl, - - -	6	0
	28	4

Three miles east of New Burlington, at about the same elevation, and several miles distant, in three wells of the vicinity were found leaves, trees, muck, and what the diggers call "grape vines." At thirty feet, a log more than a foot in diameter was struck, with knots upon it. In one of the wells, at fifteen feet, was a "dirt bed" like surface soil; in another a "swamp" at thirty feet, which ruined the water for drinking.

The land of this county lies in high long rolling hills, and the subjacent rock is the "blue limestone" of the lower Silurian.

Three miles north of New Burlington, and about two hundred feet lower than the last situation, a bed of logs was passed at thirty feet from the surface. Another at forty feet, had leaves and logs under the blue clay. Another gave the following section:—

	Feet.
1. Surface loam, - - - - -	1
2. Yellow clay, - - - - -	3
3. Sand, - - - - - -	2
4. Blue clay, - - - - - -	14
	20

5. Leaves and sticks.

In Athens county, Ohio, at the depth of forty feet, Dr. Hildreth says, logs were found in the blue hard pan or clay. At a place called Mercer, at the source of the Little Miami River, timber was found forty feet deep in the blue clay, and a dirt bed at fifty feet. In 1838, Col. John Madina of Chillicothe, showed me a piece of wood, taken as he said from a well on the hills east of the Scioto, about three hundred feet above the lake, at the depth of thirty feet. It was a foot long, one and a half inches through, hard, brittle and resinous, of a dark color, and overlaid by clay.

On the land of Mr. Simpson in Scioto county, Ohio, about ten miles east of the Scioto River, near the north line of the county,

in a dirt bed at a depth of gravel of thirty feet, was found a log in a decaying state, two feet in diameter. In Jefferson township, in the same county, three and a half miles east of the river, in a well twenty-seven feet deep, was a log which was taken for cedar.

Mr. Christy (Letter, p. 5) gives an instance of an upright tree on the land of R. Becket, Esq., eight miles east of Oxford College, Ohio, the roots fixed in the blue hard pan at a depth of thirty feet, the trunk surrounded by the "gravel drift."

In the well of the Lunatic Asylum, Columbus, Ohio, at thirty feet, after passing the yellow into the blue clay, a stick was found which was called pine. Near Loudon, Madison county, Ohio, in the blue clay beneath the yellow, timber sticks and leaves were struck at twenty-five or thirty feet. Prof. Cassells of the Cleveland Medical College, gives an instance of pine or resinous wood being found near Kendall in Stark county, Ohio, in the gravel or upper drift.

These are by no means all the instances that have come to my knowledge. They are principally from the blue clay, and are sufficiently numerous and widely diffused to establish the fact, for Ohio, that timber of a kind *now flourishing* in North America, grew, and was entombed at the oldest diluvial epoch. How the age of these deposits will compare with that of the lower part of the delta of the Mississippi, may not be stated with full confidence; but the hard pan is without much doubt the oldest.

All the cases of entombed trees which I have given, are supposed to be above the influence of our rivers as at present known, and intended to be given from above the "valley drift." I will now give a few instances near the level of large rivers, that may belong to the recent alluvium or to the "valley drift." At Marietta, between high and low water of the Ohio River, a log at thirty feet depth. At Portsmouth, at a depth of forty feet, an oak log not decayed, but of a dark blue color. This was taken out by Francis Cleveland, Esq., when constructing the Ohio Canal, from a cut across the peninsula at the mouth of the Scioto. Here at the level of low water is—

1st. Compact sand and gravel.

2d. Water-washed gravel—fifteen feet.

3d. Fine blue marly sand, with logs, leaves and sticks in immense numbers—three to twenty feet.

4th. Yellow clay, without sticks or gravel, (used for bricks)—fifteen to thirty feet.

5th. Sandy loam in ridges—surface.

No. 3 of this section is extremely like the blue marl of Lake Erie. The timber, sticks, &c. of this bed, are of kinds *now growing* in the forests of Ohio—such as beech, black ash, and oak; and are in logs fifteen inches in diameter, sometimes burnt and

charred by fire. The timber stratum extends under the town of Portsmouth, and appears on the Scioto side. On the same ground, before the occupation of white people, there were the remains of an ancient city, belonging to the "race of the mounds."

At Cincinnati, in a well near the corner of Fourth and Vine streets, Judge Burnet, who was then the proprietor, struck at the depth of ninety-three feet, a partially decayed stump, with the roots attached, standing in an upright position. The well was sunk through the water-washed sand and gravel, to the level of low water; and here as at Portsmouth, were directly over the timber, the walls, ditches and parapets of an ancient and unknown people.

I have already stated that leaves and twigs were sometimes seen in the blue marl of Lake Erie, and in the coarse sand and gravel above it.

Animal Remains.—In the tertiary strata of the Atlantic coast, of the St. Lawrence, Lake Champlain and the Hudson, of South Carolina and the lower Mississippi, the bones and grinders of the Mastodon, elephant, horse, Mylodon, Megatherium, &c., are reckoned as fossils of beds that in many respects appear to be equivalents of the hard pans of Ohio. The bones of the mastodon, elephant, beaver, horse and bison, have also been found at the west, but never in Ohio, so far as I know, *in these deposits.* They are *upon* and not *in* them. If it were not for this apparent discrepancy, I should here merely touch upon the subject. The tooth or grinder of the mastodon, found in Ohio City, and referred to by Mr. Lyell, was in the alluvium, resting upon the blue marl. The Bucyrus (Crawford Co.) mastodon, noticed by Mr. Briggs, in the Geological Report of Ohio, for 1839, was in a calcareous swamp alluvium. A tusk, and a few other bones, uncovered at the deep cut of the Mansfield railroad, a few miles from Sandusky, were in a recent bog of muck. The jaw of the "Castoroides Ohioensis" of Mr. Foster, at Nashport, was in the valley drift and swamp mud.* It was the same with a tusk found on the Ohio Canal near Massillon. Those mentioned by Mr. Lyell at Cincinnati, were in the "valley drift," and at the Big Bone Lick, Kentucky;—the bone bed is a sulphurous bog, below high water of the Ohio, and covered in places by river alluvium.

At the great collection of bones on the Osage, in Benton Co., Missouri, there appears from the descriptions to be a bed of recent bog or mire. In Deerfield township, Ross Co., Ohio, where some teeth and bones have been seen, they are in the alluvium of a brook. The bones and teeth of the horse found at Columbus, Ohio, are in the fissures or "clay seams" of the cliff limestone. The elephant skeleton disinterred by Mr. Briggs, in Salt River, Jefferson Co., Ohio, in 1838, presents a case nearer than any other

* See Ohio Report, 1838, p. 80.

in my reach, of stratified sands and clays *above* such bones; and this is at the bottom of a deep valley, with a stream at the level of the bone bed. There can be little doubt, but the beds covering the bones in this case, are newer than the "valley drift."*

These are all the locations in Ohio within my knowledge, and I think none of them can be referred to the blue or yellow hard pans, which are the beds bearing the closest resemblance to the Champlain and North River post-pliocene, of any of our diluvial deposits. The analogy, or rather the evidence of identity or equivalence, arising from the fossils, may be said therefore to fail entirely. Whether the buried timber can be regarded as a characteristic fossil, and whether in the eastern beds it exists, I cannot say. Neither can it be confidently asserted that there are no shells, or no marine shells, in the Ohio deposits, until farther examinations are made.

These facts are given free from speculations, with a view to elicit reflection and discussion among geologists, who have observed and theorized upon the diluvial matter of other parts of the United States. Perhaps there were so many different periods of diluvial action, or a different action at distant places at the same time, that the deposits in question cannot be arranged under the general law of superposition, and order as to age. If the premises I have furnished are true, a *different force* brought on the hard pans and gravel, from that of the boulders. These must have been left but a *short time,* before the recession of the waters, or they would have become partially or entirely covered with sediment. The last recession must have been easy and gradual, or it would have mingled them with the earth, on which they rest.

Connected with the movement of detrital matter, and perhaps with the transport of boulders, is the subject of the worn and striated surface of the rocks. Diluvial grooves are common in Ohio. In the few observations given below of the bearing of the grooves or scratches, only a part were determined by compass. They are made principally by the eye, upon short lines, and may vary five degrees from the true course.

Place of observation.	On what rock.	Direction.	H't above L. Erie.
Dayton, Ohio, (Dr. Locke,)	cliff limestone	{ N. 19° to 33° W. } { average N. 26° W. }	219 feet.
Lordstown, Trumbull Co.,	conglomerate	N. 20° W.	say 400 ft.
Tallmadge, Summit Co.,	coal grits	N. 30° to 40° W.	625 feet.
Canfield, Mahoning Co.,	coal grit	N. 30° E.	say 600 ft.
Newburg, Cuyahoga Co.,	grit below coal series	North	" 300 "
Sharon, Medina Co.,	conglomerate	N. 40° W.	" 600 "
Copley, Summit Co.,	"	N. 30° E.	" 600 "
Irvinsburg, "	"	N. 40° W.	" 500 "
Cuyahoga Falls,	"	N. 45° W.	" 450 "
N. Hampton, Summit Co.,	"	N. 30° to 35° W.	" 550 "
Mantua, Portage Co.,	coal grit	N. 30° to 40° E.	" 650 "
Doylestown, Wayne Co.,	"	North	600 feet.
Kelley's Island, L. Erie,	cliff limestone	Northeast by East	lake level

* See First Ohio Report, 1838, p. 96.

Of these, *seven* are west of north, *four* are east of north, and *two* in or near the meridian. There are not cases enough to deduce a good general average, where the variance is about 90°. But it is probable that future investigations will show, here as in Maine and Massachusetts, a general bearing to the *west* of *north*, 10° or 15°. In western New York, the average direction is to the *east* of north, about 20°.

To determine the elevation of places in the above table from the ocean, add 564 feet, the ordinary level of Lake Erie.

ART. XXIV.—*A Memoir of Dr. Douglass Houghton;* late State Geologist, and Professor of Geology and Chemistry in the University of Michigan.

THE life of the man of science belongs to the age. If that life has been illustrated by great talents and virtues, not merely will an interest attached to its history, but its memory will awaken a deeper love of science in the breasts of her votaries, and a more eager inquiry among the uninstructed, into those truths which could attract the minds of such men and bring out their good qualities.

Such a life was that of Dr. Houghton. He was born at Troy, in the State of New York, Sept. 21, 1809, but at an early age accompanied his father's family to Fredonia, in western New York. In this quiet village, and during the usually thoughtless years of childhood, he laid the foundation of his scientific reputation. A little hermitage erected by himself in the shade and seclusion of his father's orchard, for a place of quiet study, still remains, a witness to the self-directed character of his acquirements. Here he commenced his researches into the laws of nature. Nor was the mere acquisition of knowledge all that attracted his mind; he at once turned it to practical account. At this period he commenced experiments in the manufacture of percussion powder, which had been then recently invented, and he had actually proceeded some way in the erection and management of a manufactory for the purpose, when an unlucky explosion took place, by which the whole works were blown up, and young Douglass along with them. The latter chanced to alight on a neighboring shed, and when returned to his senses, found his clothes in a blaze. He had the presence of mind to plunge instantly into a pond, and thus prevented further injury, but he bore evidences of the accident ever afterwards. With all this love of studious application, Douglass was the very "head and front" of all the fun making exploits of the village boys, of whom he was the favorite. Active and persevering, possessed of great natural wit and shrewdness, he was the acknowledged leader in boyish enterprises.

Thus passed his boyhood. Before its ordinary period was elapsed, a career of honorable distinction opened to him. His talents and studious habits led to his selection from the county of Chautauque, to be educated at the Rensselaer High School, at Troy, at that time under the charge of Prof. Amos Eaton, and enjoying that high reputation which, under his direction, it sustained for so many years. Here he graduated and received his diploma as Bachelor of Arts, in Oct., 1829. He remained as an assistant instructor, and in February of the year following, received from Prof. Eaton the appointment of Assistant Professor in the branches of Chemistry and Natural History. Prof. Eaton stated to the Board that this arrangement was temporary, for he was sure Mr. Houghton would receive a higher appointment as soon as the proper authorities could act.

In Nov., 1830, some friends of science in Detroit applied to Prof. Eaton to recommend them a person qualified to deliver a course of public lectures on chemistry, geology, mineralogy and natural philosophy. The Hon. Lucius Lyon was deputed to wait in person upon the Professor. Having heard the request, Prof Eaton opened the door of the laboratory, where young Houghton was engaged, and calling him by his familiar name of Douglass, presented him to Mr. Lyon, as the person he required. Small and extremely youthful in appearance, it was some time before Mr. Lyon could be satisfied that the Professor was in earnest in this recommendation. But these doubts were speedily removed, and well did the young candidate justify the opinion of his instructor and friend. Still retaining his professorship, he gladly obtained leave of absence, and immediately departed for Detroit. With his lectures there, commenced his career in Michigan. It is no small testimony to his merit, that at a gay military post, devoted to amusement, and at an age when few have acquired the art of imparting as well as receiving instruction, he succeeded in captivating all hearts to his favorite pursuits. His lecture soon became the preferred resort, even of those who had been accustomed to look for enjoyment only in balls and pleasure parties.

Having pursued the study of medicine, Mr. Houghton was, in the spring of 1831, admitted to practice, by the Medical Society of Chautauque Co. At this period he also received the appointment of Physician and Botanist to the expedition to the sources of the Mississippi, organized under the direction of Henry R. Schoolcraft, Esq., and his able report on the botany of that remote country not only shewed an extensive acquaintance with that science, but his researches did much to extend our knowledge of the flora of the Northwest, as well as to establish his own reputation.

From 1832 to 1837, Dr. Houghton practiced as a physician and surgeon at Detroit. This period included the visitation of the cholera, which raged at that place with more than its wonted severity. That event will never be forgotten by the survivors; nor will they forget the labors and devotion of Dr. Houghton. He was to the sick a ministering angel, prompt at a moment's notice, instant and active in his remedies, cheerful and obliging, as well to those from whom he expected no pay, as to the opulent.

In Feb., 1837, Dr. Houghton projected the geological survey of Michigan, and received the appointment of geologist to that state, which, but recently admitted into the Union, and as yet sparsely populated, was one of the foremost in public enterprise.

The plan upon which this survey was organized was such as reflected great credit upon the enlarged scientific views and enterprise of Dr. Houghton, and may be considered as a model. It comprehended four departments, viz., geology and mineralogy proper, zoology, botany, and topography; each having its official head and assistance, and all united under the general direction and surveillance of the state geologist. Thus while all were expected to work in concert, by a division of labor, the results would necessarily be more extended and accurate. One of the duties connected with the topographical department may be considered as novel to a geological survey, and could at least be but ill accomplished in any of the older states. Like all the states northwest of the Ohio, the system of rectilinear surveys of the United States had been applied to Michigan; by which the state is divided into towns and ranges of six miles square; which, again are subdivided into thirty-six square miles or sections. The topographer was directed to furnish the geologist and his two assistants in the geology proper, with 'skeleton plats' of these townships, copied from the returns of the deputy U. S. surveyors, on a scale of two inches to the mile; these serving the latter as a basis for laying down, with more than ordinary accuracy and facility, as well the geological as the topographical and civil features of the country. These being returned to the topographer, were reduced by him to the scale adopted for a series of state and county maps, the publication of which was projected as a part of the results of the survey. The condition of the state finances has permitted the issuing of a few only of the maps thus prepared, but they are sufficient to shew, that for comprehensiveness, minuteness and fidelity of detail, these maps of the several counties of Michigan far exceed any thing of the kind ever attempted in this country.

The cause adverted to also proved fatal to the departments of zoology and botany, before the close of the second year of their complete organization; so that Michigan, unfortunately, lost the

opportunity of prosecuting hand in hand with its geology, those kindred sciences, at a time when the country yet preserved many of those plants and animals that become extinct with the progress of settlements. The comprehensive mind of the geologist regarded these sciences as part of the one great science of nature, and lending mutual aid.

The season of 1840 was passed by the geological corps in an exploration of the southern coast of Lake Superior, and the general results were reported by Dr. Houghton to the legislature the winter following. Though displaying the most guarded caution, and "telling but half the tale" which the field notes unfolded, this report brought to notice such an array of facts, bearing upon the mineral wealth of that country, so philosophically deduced, and carrying such order and system into what all previous observers had regarded as unintelligible confusion, that public attention was immediately attracted to that region, and thousands were soon seeking their fortunes there, who had joined in the cry that "all was barren." The novelty of the geological positions advanced, and their apparent unconformity, in many particulars, with the state of facts existing in other well known mining districts, created only incredulity in those who had received their teaching in other schools of science. The Doctor was styled, in derision, a "back-woods geologist." But the progress of discovery since, though collected from numerous and independent sources, and opened to the scrutiny of many of the best scientific observers of the age, has confirmed the positions assumed by him; while so far from discrediting a single fact advanced, but little has been added even to those already made known.

Dr. Houghton's labors in developing the geology of Michigan, a state at that time in great part a wilderness, were necessarily arduous, and they were unremitting. The nature of these difficulties, and how they were endured, is best told in the language of one of his own reports. "Wading the streams by day, annoyed by mosquetoes by night, separated for weeks together from all society, were it not that the mind is constantly occupied in the contemplation of objects, which from their symmetry and beauty, furnish a constant mental feast, there would be nothing which could possibly compensate for the hardships endured." To feel the full force of this observation, we must also take into consideration that for a considerable portion of the time, the annual appropriation was scarcely adequate even to the travelling expenses of those engaged. It was no closet study which brought out the results thus developed. Few scientific men could have ventured upon such a task, and it would have required all the energy, perseverance, and bodily as well as mental vigor which characterized Dr. Houghton, even with ample means to accomplish it.

The personal appearance of the Michigan geologist, while engaged in these wild wood journeyings, was unique in the extreme. Neat in dress at home, he well knew how to throw aside those restraints that were incompatible with the wilderness. His short figure, enveloped in a loose frock of sheeps-gray or bedtick; his "shocking bad" broad brimmed, wool hat, and boots matched in breadth of sole; his beard, for months unacquainted with a razor, and pendant from his chin in a pointed brush of colorless bristles, presented a ludicrous but characteristic spectacle.

The United States linear surveys were in progress in Michigan, and great advantages were derived to the work in which Dr. Houghton was engaged, by enlisting the minds of the surveyors in the subject of the geology, and making use of their notes and observations. It was thus that he derived the idea of accomplishing a thorough geological, mineralogical, topographical, and magnetical survey of the new lands of the United States, contemporaneously with the government surveys. As the act making provision for the state geological survey had expired, leaving still a large territory in the upper peninsula unexplored, he set about perfecting the plan which he had previously conceived of connecting the lineal surveys with a minute geological and mineralogical survey of the country. Having explained his system at one of the annual meetings of the Association of American Geologists and Naturalists, he, as chairman of a committee from that body, visited Washington during the session of Congress in the winter of 1844, and laid his plan before the proper department. The feasibility of the project was at once comprehended. The only doubt raised was whether deputy surveyors could be found who were sufficiently versed in the natural sciences, to undertake the work. This apparent obstacle was instantly removed, by the offer of Dr. H. to take the contract for running of four thousand miles of lines, at a price but little if any exceeding that which would have been paid for a single survey. His confidence in the plan which he had projected was no longer a matter of doubt, when he was willing to take upon himself the hardship and excessive labor incident to the survey of a new, distant, and uninhabited country. But hardship and labor had no terrors to him, in the prosecution of his favorite studies. He was no mere theoretical geologist. He loved better to study the operations of nature in her own workshop, than to read the theories of any man in his parlor. His mind could be satisfied only when he had put every thing into the crucible and tested it for himself.

The system proved signally successful. The present surveyor general for this district, the Hon. Lucius Lyon, who was in Congress at the time the plan was originated, says in his report to the Commissioner of the General Land Office, in 1845, speaking of this survey, "that enough has been done to shew clearly the

great value of such accurate geological and topographical surveys as the one in which he was engaged, and to demonstrate the practicability of carrying them on in connection with the ordinary linear surveys of the public lands, without increasing the expenditures of the government for surveying more than half a cent per acre, even in a rough and thickly wooded country like that on Lake Superior."

"The additional information which such surveys would give," (if generally adopted,) "would certainly be worth to the purchaser of the public lands far more than this small sum in any part of the United States; and in a mining region its value and importance would be greatly increased. It would enable the government to know at once the exact location and probable value of every section and quarter section of the mineral lands."

This is the opinion of one who, besides the position he holds, is himself a practical surveyor and for a long period has made geology and mineralogy his favorite studies.

At the session of the legislature of Michigan, next succeeding the untimely death of Dr. Houghton, a joint committee of both houses was appointed to examine the condition of the survey. In speaking of the system "devised and thus far successfully prosecuted by him," they say it was one "which at little additional expense over that of the ordinary surveys of the United States, combines, by the simplest and cheapest means, all the advantages of the immensely expensive surveys by triangulation and instrumental observations, carried on by many of the governments of Europe."

The vast amount of information returned to the office of the Surveyor General, through this survey, will enable him at once, when the lands on Lake Superior are offered for sale, to determine, with the utmost possible accuracy, what should be designated as mineral and what as farming lands. Its extent and minuteness will be comprehended, when we state, that there is not a spot through the whole extent of the vast region thus far included in these surveys, that has not been actually visited, or been approached within the distance of half a mile, by men well qualified to judge of the geology and mineralogy of the country; that at least one specimen is collected illustrative of every rock met with, and its place noted by its chains and links; that the complete topography of the country is exhibited, in the same manner, in the plots and notes, and that every variation of the magnetic needle is also noted upon the lines. In addition to which, the superintending geologist, with the aid of these sources of information, is enabled, from any desirable point, to trace the boundaries and extent of the several formations, the course and character of veins, and to connect all these details with other matter of general geological interest. The difference between

such a system, and the usual expensive, disconnected, and comparatively uncertain and partial method of geological surveying, will be readily perceived. By the former definite and practical system, the surveys of the United States have been brought to a state of perfection, which comprehends all that a governmental survey could require.

Under the able direction of the Surveyor General for this district, this system of surveys was continued, after the death of Dr. Houghton, with such change only as experience suggested, until the past season, when it was suddenly abandoned by the government, and the usual expensive and unsatisfactory system introduced. We have alluded to this subject thus fully, both because it seemed due to the reputation of Dr. Houghton, and because we believe it could not have been abandoned had the subject been examined by the head of the Treasury Department with the care and ability for which he is distinguished.

Dr. Houghton having entered this new field, had in contemplation a project which if carried into execution would have done much towards perfecting our knowledge of the geology and mineralogy of the United States. Under his system the General Land Office at Washington would have received a vast number of specimens and an amount and variety of information never before conceived. It would in time have rendered desirable a department where this information could be condensed and placed before the public in the most connected and practical form. In other words, it was in his opinion the nucleus of a scientific bureau, where not only all the information acquired in the survey of the vast country extending from the Mississippi to the Pacific Ocean, would be accumulated, but where would be found all the discoveries made by the several states in the prosecution of their surveys. Such were the plans which Dr. Houghton had formed, and which his death interrupted. They were worthy of his genius, and we trust will not be permitted to slumber, by the scientific men of the Union.

As the demise of Dr. H. prevented his making his final report upon the geology of Michigan, it is further due to his memory to state, that the amount of materials collected, in the form of notes, plots, geological, mineralogical, botanical and zoological specimens, and engravings, and which now enrich the cabinets of Michigan, probably surpass those obtained by any of the other states, New York excepted; and it is almost needless to say that the results are second in interest to none.

In 1838, Dr. H. received the appointment of Professor of Geology, Mineralogy and Chemistry in the University of Michigan; which he continued to fill till the time of his death. The institution was then in its infancy, and Prof. Houghton's labors being required elsewhere, he was enabled to devote but little time to

the duties of the professorship. His influence was however of great service to the rising institution.

The ambition of Dr. Houghton and his popularity in the state led him to attempt other fields of distinction. He knew how the political wires were worked, and though he permitted himself to enter that difficult and harassing arena, only in the latter period of his life, he yet came sufficiently "before the public" to evince that he possessed, in a high degree, the talents, which in this country are supposed to be necessary to the statesman. As Mayor of the city of Detroit he acted independent of party or clique, and was at once energetic, keen sighted and assiduous. Of his other political connections, which were cut short by his death, we will only say, that it was the desire and expectation of his numerous friends to see the gubernatorial chair of the state filled by one who would so well have honored that station.

Dr. Houghton was married in 1833, to Miss Stevens, of Fredonia, and his death bereft also two children. But for his faithful devotion to the duties of his public station, he would undoubtedly have left his family in affluence, since his success in the business operations in which he engaged showed that in these matters also he possessed a vigilant eye and correct judgment.

Dr. Houghton was about five feet three inches in height. He had a remarkably animated blue eye, and his light hair and complexion indicated a sanguine nervous temperament.

The mind of Dr. Houghton had that preëminent stamp of genius that it was equally great in whatever he undertook. Whether, amid the rocks and wilds of the West, he was solving some intricate problem of nature, or at his desk directing the ordinary concerns of business, he applied to all alike the most active energies of his intellect. He thought nothing beneath his notice from which any thing might be learned, and he uniformly obeyed the maxim that "what is worth doing is worth doing well." He thus became the *man of resources*, both from the amazing extent of his acquaintance with the details of the arts, and from his power of combining and applying this knowledge under varied circumstances.

If in so utilitarian a country as ours, this practical tendency can be carried too far, it was so in Dr. Houghton; for though it did not destroy his appreciation of what may be called the "poetry of nature" and the beautiful in art, it diminished his taste for them, and rendered him less susceptible to their sympathetic influences. It was a frequent observation with him that the usual college course was ill fitted for the proper education of the useful man. He would have the inventions of men less studied, the works of nature more.

Dr. Houghton's powers of perception were wonderfully rapid. He could with the celerity of thought seize upon the strong

points of a subject, and thus acquire a knowledge of its character and general bearings in a wonderfully short time. This is perhaps almost a necessary power of genius, that while ordinary minds are toiling laboriously to acquire the mass of heterogeneous materials in which truth is often involved, the former grasps at the great leading facts, and thus forms a base around which minor facts can be subsequently built up, by the cement of association, until the structure is perfected.

In moments of relaxation, Dr. Houghton's mind turned rather to the ludicrous than the pathetic or ideal. Sometimes, with a singularly rapid transition, he passed from the contemplation or exposition of the most abstract truths of science, to the utterance of a jest. Of humorous stories he had a great fund, and was particularly happy in his manner of relating them. What added to the charm of these comic episodes, was the interest he himself manifested in them. Nothing could be more contagious than the sudden hilarious laugh with which he welcomed any little piece of fun or frolic. It was irresistible, and would sometimes set a whole company into a roar, when few of them comprehended the cause of the universal merriment. Thus, though far from fluent, his social and conversational powers were of no ordinary kind—whether he drew upon his mind as a vast storehouse of useful knowledge, or for that fund of wit and vivacity which made him the delight of the convivial circle.

In manner, Dr. Houghton was exceedingly courteous, never allowing the most pressing engagements to interrupt, in appearance at least, the cordiality with which he greeted a visitor. To the poor and even the low bred, he was ever affable and obliging. When such called at his office for advice or medicine, he never showed by his manner that he wished to be rid of them as soon as possible, and grudged the time or medicine he bestowed. On the contrary, he inquired, in a manner which such best can feel, after the various matters of health, family or business in which they felt most interest, and when opportunity occurred, he did not fail to add more substantial proofs of kindness. Thus amid the high calls of duty and fame, these acts of private benevolence gained him as many warm friends among those who could little appreciate his public worth, as did his distinguished abilities among those who knew him only as a public man.

How much importance he attached to a respectful demeanor in his intercourse with people, the following incident may illustrate. Travelling once with a younger friend, they had occasion to enquire their road, when his companion called to a lad whom they met, addressing him as 'boy.' When they had passed on, the Doctor mildly corrected him, by saying, "Always address such a person as 'young man'—it appeals to his self-respect."

To these qualities of mind and heart, Dr. Houghton added a wonderful degree of resolution and energy, amounting almost to a fault. Having fixed his aim, he shrunk from no difficulties, and seldom failed to accomplish what he undertook. Cautious and analytical, prejudice was seldom allowed to close his mind against conviction. The result was, he not only arrived at accurate conclusions, but far in advance of those who were either too biased or too indolent to investigate.

Thus fitted by constitution and education for a province in the cause of knowledge, in the new and great West, and ardently attached to the fortunes of his adopted home, his active spirit launched him, like another Humboldt, upon its almost boundless wilderness, and waters magnificent in their vastness and solitude, to explore these yet unread pages of the great book of nature. In the prosecution of these labors, to which with rare devotion he contributed his time, his purse and his health, he succeeded, even in his unfinished task, in building up an envied reputation among the scientific men of the age, and a distinguished name among those whom his state would delight to honor.

From these a mysterious providence called him away; the work of laborious years unfinished; the full harvest of his honors unreaped. The circumstances attending his melancholy end are characteristic both of his nobleness and his intrepidity. He was engaged upon his new survey on Lake Superior. Accustomed to steer his own boat, fearless in his knowledge of the waves and of the coast, and anxious to arrive at a particular destination, he paid too little heed to the omens that warned his Canadian voyageurs of the storm; or rather, he trusted, as he had done a hundred times before under circumstances of greater apparent peril, to his own judgment and skill, to reach the port before the danger should be imminent. When the storm commenced, his men proposed going ashore; Dr. Houghton encouraged them to proceed, saying, "We are not far from Eagle River, (their destination,)—pull away my boys—we shall soon be there—pull hard." When in the increasing violence of the gale, the frail boat encountered the surf and was capsized, the geologist was raised from the water by his trusty voyageur, Peter, and told to cling to the keel. He answered, "Never mind me, Peter, go ashore if you can—I will go ashore well enough." Another sea struck the boat, throwing it completely over endwise, and probably occasioning instant strangulation. This was on the night of the 13th of October, 1845.

Thus, at the early age of thirty-six years, was lost to his state and science one who, without eulogy, may be ranked among the most extraordinary men of our country; whether we view him as the humble student of nature, attracting all hearts to science; the friendly and skillful physician, periling his life to save that of

others; the energetic and independent public man, untiring in his energy, and sacrificing his private means in the public cause; or the universal instructor of youth and age, the source of as frequent and general reference as the pages of a cyclopedia.

The soldier dies honored who falls in battle. He too, perished "on the field of his fame"—a field whose victories are bloodless, and in whose fruits, untainted by misery and crime, the whole human family may rejoice.

ART. XXV.—*The Sphere and least Circumscribing Cone;* by Prof. E. S. SNELL, of Amherst College.

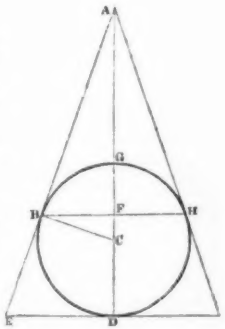
IN searching for practical examples of maxima and minima, for my pupils in the Differential Calculus, I have fallen upon the following relations of the sphere to its circumscribing cone, which are very singular, and to me entirely new.

1. The cone of least *surface*, which circumscribes a given sphere, is also the cone of least *volume*, circumscribing the same sphere.

2. This minimum cone has *twice* the *height* of the sphere, *twice* its *surface*, and *twice* its *volume*.

3. The base of the same cone equals the surface of the hemisphere; and the convex surface of the cone, three times the surface of the hemisphere.

Demonstration.



In the accompanying sectional figure, which it is unnecessary to describe, let AC $= x$, and BC or CD $= r$; then the height of the cone is $x+r$. The similar triangles, ABC and ADE, give $DE = \frac{r(x+r)}{\sqrt{x^2-r^2}}$; hence the base of the cone is $\frac{\pi r^2(x+r)^2}{x^2-r^2}$, and its solidity is $\frac{\pi r^2(x+r)^3}{3(x^2-r^2)}$. To facilitate the differentiation of this function, we divide by the constant $\frac{\pi r^2}{3}$, and reduce to the lowest terms; when our equation may be expressed, $V = \frac{(x+r)^2}{x-r}$. The first and second differentials are,

$$\frac{dv}{dx} = \frac{2(x+r)(x-r)-(x+r)^2}{(x-r)^2}, \text{ and } \frac{d^2v}{dx^2} = \frac{8r^2}{(x-r)^3}.$$

Putting the first $= 0$, and reducing, we find $x = 3r$; therefore the height of the cone is $4r$, or twice the height of the sphere.

By substituting $3r$ for x in the expression for the cone, $\frac{\pi r^2(x+r)^3}{3(x^3-r^3)}$, it becomes, when reduced, $\frac{\pi}{3}\cdot(2r)^3$. But the sphere is $\frac{\pi}{6}\cdot(2r)^3$. Therefore the volume of the cone is twice that of the sphere.

The same substitution being made in the value of $\frac{d^2v}{dx^2}$, it becomes $+\frac{1}{r}$, indicating that the cone is a minimum, which is indeed sufficiently evident from the nature of the question.

Another proportion on the triangles ABC and ADE, gives the slant height $= \frac{x(x+r)}{\sqrt{x^2-r^2}}$. And since the radius of the base is $\frac{r(x+r)}{\sqrt{x^2-r^2}}$, the circumference is $\frac{2\pi r(x+r)}{\sqrt{x^2-r^2}}$; and therefore the convex surface $= \frac{\pi r x(x+r)^2}{x^2-r^2} = \frac{\pi r x(x+r)}{x-r}$. This added to the base, $\left(\text{which equals } \frac{\pi r^2(x+r)}{x-r},\right)$ gives for the entire surface of the cone, $\frac{\pi r(x+r)^2}{x-r}$. The first and second differentials, after dividing by the constant πr, are, $\frac{ds}{dx} = \frac{2(x+r)(x-r)-(x+r)^2}{(x-r)^2}$, and $\frac{d^2s}{dx^2} = \frac{8r^2}{(x-r)^3}$. As these are precisely like the former, they lead to the same conclusion, that x equals $3r$, and corresponds to a minimum value of the function. Therefore, the circumscribing cone of least *surface* also, has twice the altitude of the sphere, and is identical with the cone of least *volume*. To obtain the amount of surface, replace x by $3r$ in the expression $\frac{\pi r(x+r)^2}{x-r}$, and it becomes $2\pi(2r)^2$. Now the surface of the sphere is $\pi(2r)^2$. Wherefore the cone of least surface about a sphere, has twice the surface of the sphere.

These results brought together are as follows:

	Sphere.	Min. Cir. Cone.
Altitude, - -	$2r$ - -	$4r$
Surface, - -	$\pi(2r)^2$ - -	$2\pi(2r)^2$
Volume, - -	$\frac{\pi}{6}(2r)^3$ - -	$\frac{\pi}{3}(2r)^3$

We have seen that the convex surface $= x \cdot \frac{\pi r(x+r)}{x-r}$,

and that the base $= r \cdot \frac{\pi r(x+r)}{x-r}$;

and since $x = 3r$, the convex surface is three times the base, and the base, consequently, one fourth of the entire surface of the cone. Therefore the base of the cone is equal to the surface of the hemisphere, and the convex surface, to three times the same. Hence it is obvious that the ratio of 3 to 1 cannot belong to the surfaces of any cone, which is not a minimum among those that circumscribe the same sphere; since any change of form in the cone, involves a change of ratios between its convex surface and base.

These results are so remarkable, so elegant, and obtained with such ease, that it seems scarcely possible they should have escaped observation; but I have looked in vain for any trace of them, in those mathematical works to which I have access.

I will notice but one other circumstance relating to these bodies; which is, that just *one-third* of the spheric surface is cut off on the upper side by the circle of contact. For AC : BC :: BC : FC; therefore FC is one-third of GC, and GF one-third of GD, and hence the surface BGH is one-third of the entire spheric surface. It appears, then, that if a sphere be viewed from a point, whose distance from its surface equals its diameter, *one third* of the surface is visible.

As this is one instance of the application of a very simple formula, for determining the amount of spheric surface visible at any given distance, I will take this opportunity of presenting it. If the number of diameters expressing the distance, be made the numerator of a fraction, whose denominator is twice the same number plus one, this fraction will express the portion of surface visible at that distance. Thus, at the distance of

1	diameter,	$\frac{1}{3}$	is seen,
2	"	$\frac{2}{5}$	"
3	"	$\frac{3}{7}$	"
10	"	$\frac{10}{21}$	" and generally,
x	"	$\frac{x}{2x+1}$	" whether x be integral or fractional;

approaching nearer one-half, as the distance increases, without the possibility of ever equaling it. As the demonstration of this formula is only a generalization of the example already given, I omit it.

The earth is about 115 lunar diameters from the moon, and therefore we can see $\frac{115}{231}$ $(=\cdot498)$ of her entire surface; while, from the moon, only $\frac{40}{81}$ $(=\cdot491)$ of the earth's surface can be seen. Of the solar surface, on the other hand, we have about ·498 in view, while ·49998 of the earth's surface is visible from the sun. So an observer, elevated one mile from the earth, sees $\frac{1}{8000} \div (\frac{2}{8000} + 1)$ of its surface, $= \frac{1}{8002}$, or about 25,000 square miles.

ART. XXVI.—*Objections to the Theories severally of Franklin, Dufay and Ampère, with an Effort to explain Electrical Phenomena by Statical or Undulatory Polarization*;* by ROBERT HARE, M.D., Emeritus Professor of Chemistry in the University of Pennsylvania.†

1. IT appears, from the experiments of Wheatstone, that the discharge of a Leyden jar, by means of a copper wire, takes place within a time so small, that were the transfer of a fluid from the positive to the negative surface requisite for its accomplishment, a current having a velocity exceeding two hundred thousand miles in a second would be necessary.

2. The only causes for the velocity of an electric current, according to Franklin, are the repulsion between the particles of the electric fluid, of which it has been assumed to consist, and the attraction between these particles and other matter. These forces are alledged to concur in distributing the supposed fluid throughout space; whether otherwise void, or partially occupied by conducting solids or fluids. Hence, when between two or more spaces, surfaces, or conducting masses, there is an unequal distribution of the electric fluid, the equilibrium is restored whenever a communication is opened, by means of a sufficiently conducting medium. Agreeably to this view of the subject, there seems to be a resemblance between the supposed effort of the electrical fluid to attain a state of equable diffusion, and that which would exist in the case of a gas confined in adjoining receivers, so as to be more dense within one than within the other; for, however the subtilty of the supposed electric fluid may exceed that of any gas, there seems to be an analogy as respects the processes of diffusion which must prevail. But on opening a communication between cavities in which any aëriform fluid exists, in different degrees of condensation; evidently there must be a diminution of density, and consequently of velocity, in the resulting current, proportionally as the excess diminishes; so that

* Agreeably to Faraday's researches, and general experience, we have reason to believe that all particles of matter are endowed with one or the other of two species of polarity. This word polarity conveys the idea that two terminations in each particle are respectively endowed with forces which are analogous, but contrary in their nature; so that of any two homogeneous particles, the similar poles repel each other, while the dissimilar attract; likewise when freely suspended they take a certain position relatively to each other, and on due proximity, the opposite polar forces, counteracting each other, appear to be extinct. When deranged from this natural state of reciprocal neutralization, their liberated poles react with the particles of adjacent bodies, or those in the surrounding medium. Under these circumstances, any body which may be constituted of the particles thus reacting, is said to be polarized, or in a state of polarization.

† Read before the Academy of Natural Sciences, and with their permission published in the Medical Examiner: republished for the use of medical students, with corrections and additions by the author.

the difference between the initial and final velocity, must be immense. Far from taking place in an analogous manner, electrical discharges are effected with an extreme suddenness, the whole of the redundancy being discharged at once, in a mode more like the flight of a bullet, projected with infinite velocity, than that of a jet varying in celerity from a maximum to a minimum.

3. So far, in fact, is an electrical discharge from displaying the features which belong to the reaction of a condensed elastic fluid, that agreeably to the observations of our distinguished countryman Henry, the result is more like the vibrations of a spring, which, in striving to regain its normal position, goes beyond it. The first discharge between the surfaces of a Leyden jar is not productive of a perfect equilibrium. The transfer of different polarities beyond the point of reciprocal neutralization, producing a state, to a small extent, the opposite of that at first existing; and hence a refluent discharge ensues, opposite in direction to the primary one. But even this does not produce an equilibrium, so that a third effort is made. These alternate discharges were detected by means of the magnetism imparted to needles exposed in coils of copper wire.*

4. Supposing one row or more of electrical particles, forming such a filament of electricity, as must occupy the space within a wire of great length, to be made the medium of discharge to a Leyden jar; agreeably to the hypothesis of one fluid, the electrical filament must be attracted at one end of the wire, and propelled at the other, as soon as its terminations are brought into due communication with the coatings of the jar. Yet the influence of the oppositely charged surfaces of the jar, cannot be conceived to extend to those portions of the electricity which are remote from the points of contact, until they be reached by a succession of vibrations. Hence it is inconceivable, that every particle in the filament of electric matter can be made at the same time to move, so as to constitute a current having the necessary velocity and volume to transfer, instantaneously, the electricity requisite to constitute a charge. Even the transmission of the impulses, in such an infinitesimal of time, seems to be inconceivable.

5. In reply to these objections, it has been urged by the Franklinians, that a conductor being replete with electricity, as soon as this fluid should be moved at one end, it ought to move at the other. This might be true of a fluid if incompressible, but could not hold good were it elastic. A bell wire moves at both ends when pulled only at one; but this would not ensue were a cord of gum elastic substituted for the wire.

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* See my Treatise of Electro-magnetism, page 116.

6. But if the flow of one fluid, with the enormous velocity inferred, be difficult to conceive, still more must it be incomprehensible that two fluids can rush with similar celerity, from each surface of the jar, in opposite directions, through the narrow channel afforded by a wire; especially as they are alledged to exercise an intense affinity; so that it is only by a series of decompositions and recompositions that they can pass each other. That agreeably to the theory of Dufay, equivalent portions of the resinous and vitreous fluids must exchange places during an electrical discharge, will appear evident from the following considerations:

7. One surface being redundant with vitreous and deficient commensurately of resinous electricity, and the other redundant with the resinous and deficient of the vitreous fluid, it is inevitable that to restore the equilibrium, there must be a simultaneous transfer of each redundancy to the surface wherein there is a deficiency of it to be supplied. If after decomposing a large portion of the neutral compound previously existing on the surfaces of the jar, and transferring the ingredients severally in opposite directions, so as to cause each to exist in excess upon the surface assigned to it, were the redundancies, thus originated, to be neutralized by meeting in the discharging rod, neither surface could recover its quota of the electrical ingredient of which it must have been deprived agreeably to the premises.

8. This calls to mind the fact, that no evidence has been adduced of the existence of any tertium quid, arising from the union of the supposed electricities, founded on any property displayed by their resulting combination in the neutral state. It must, if it exist, constitute an anomalous matter, destitute of all properties, and of the existence of which we have no evidence, besides that founded on the appearance and disappearance of its alledged ingredients.

9. But however plausibly the discharges consequent to making a conducting communication from one electrified mass or surface to another mass or surface in an opposite state, may be ascribed to accumulations either of one or of two fluids; neither, according to one theory nor the other, is it possible to account satisfactorily for the stationary magnetism with which steel may be endowed, nor the transitory magnetism, or dynamic power of induction, acquired by wires transmitting galvanic discharges.

10. For the most plausible effort which has been made for the purpose of reconciling the phenomena of electro-magnetism with the theory of two fluids, or with that of one fluid, so far as these theories are convertible, we are indebted to Ampère.

11. According to the hypothesis advanced by this eminent philosopher, the difference between a magnetized and an electrified body* is not attributable to any diversity in the imponderable

* See Compendium—Electro-magnetism, paragraph 722.

matter to which their properties are respectively due, but to a difference in the actual state or distribution of that matter. Statical polarity is the consequence of the unequal distribution of the two electric fluids whose existence he assumes; while magnetical polarity is the consequence merely of the motion of those fluids, which, in magnets, are supposed to gyrate in opposite directions about each particle of the mass. These gyrations are conceived to take place only in planes at right angles to the axis of the magnet; so that, in a straight magnet, the planes of the orbits must be parallel to each other.*

12. The aggregate effect of all the minute vortices of the electrical fluids, in any one plane, bounded by the lateral surfaces of the magnet, is equivalent externally to one vortex, since, in either case, every electric particle on that surface will so move as to describe tangents to a circle drawn about the axis of the magnet. When the electrical vortices of the pole of one magnet conflict in their direction with those of another, as when similar magnetic poles are approximated, repulsion ensues; but if the vortices are coincident in direction, as when dissimilar poles are near, attraction takes place. When a current through a galvanized wire† concurs in direction with the magnetic vortices, as above described, attraction ensues; repulsion resulting when it does not so concur. Hence, the magnet, if moveable, will strive to assume a position in which its electrical currents will not conflict with those of the wire on one side more than on the other: also the wire, if moveable, will tend so to arrange itself as to produce the same result, which can arrive only when the needle is at right angles to the wire, and its sides consequently equidistant therefrom.

13. Electric currents will produce magnetic vortices, and, reciprocally, magnetic vortices will produce electric currents. Hence the magnetism imparted to iron by galvanized spirals, and the Faradian currents produced by magnetized iron within spirals not galvanized.

14. Ampère's theory has, in a high degree, the usual fault of substituting one mystery for another; but, on the other hand, it has, in an equally high extent, the only merit to which any theory can make an indisputable claim: I mean, that of associating facts so as to make them more easy to comprehend and to remember, enabling us, by analogy, to foresee results, and thus affording a clue in our investigations. Evidently, the author of this theory was guided by it, in his highly interesting and instructive contrivances; and Professor Henry ascribes his success in improving

* The words gyration, vortex and whirl, are considered as synonymous, and used indifferently to avoid monotony.

† I consider a wire as galvanized, when it is made the medium of the discharge from a galvanic battery.

the electro-magnet, to the theoretic clue which he had received from Ampère.*

15. Nevertheless, the postulates on which this Ampèrian hypothesis is founded, appear to me unreasonable. They require us to concede that about every atom of a permanent magnet a process is going on, analogous to that generally admitted to exist in a galvanic circuit, where two fluids pass each other in a common channel by a series of decompositions and recompositions. In the galvanic circuit this process is sustained by chemical reaction; but without any co-enduring cause, how is it to be sustained permanently in a magnet? Is it reasonable to assume that the heterogeneous constituents of an imaginary tertium quid are perpetually separating only to reunite?

16. In cases of complex affinity, where four particles, A B C D are united into two compounds A B, C D, it is easy to conceive that, in obedience to a stronger affinity, A shall combine with C, and B with D: but, without any extraneous agency, wherefore, in any one compound, should a particle A quit one particle B, in order to unite with another particle of the same kind; or wherefore should any one, B, quit one A, in order to combine with another A.

17. That such a process should take place in consequence of the inductive agency of a similar process already established in a magnet or galvanized wire, were difficult to believe; but it would seem utterly incredible that the most *transient* influence of such induction, should be productive of such permanent electrolytic gyration as has been above specified. Moreover, it is inconceivable that the particles of any matter should, as required by this hypothesis, *merely by being put into motion*, acquire a power of reciprocal repulsion, or attraction, of which it were otherwise destitute.

18. The vortices being assumed to take place about each atom, cannot severally occupy an area of greater diameter than can exist between the centres of any two atoms. Of course, the gyratory force exercised about the surface of a magnet by the aggregate movements of the vortices, cannot extend beyond the surface more than half the diameter of one of the minute areas of gyration alluded to. Wherefore, then, do these gyrations, when similar in direction, from their concurrence approach each other; when dissimilar in direction from contrariety, move away, even when situated comparatively at a great distance?

19. I should consider Ampère's theory as more reasonable, were it founded upon the existence of one fluid; since, in that case, vortices might be imagined without the necessity of supposing an endless and unaccountable separation and reunion of two sets

* See my Compendium, Treatise of Electro-magnetism, page 120.

of particles; not only devoid of any property capable of sustaining their alleged opposite gyrations, but actually endowed with an intense reciprocal attraction which must render such gyrations impossible. But even if grounded on the idea of one fluid, this celebrated hypothesis does not seem to me to account for the phenomena which it was intended to explain. If distinct portions of any fluid do not attract or repel each other when at rest, wherefore should they either attract or repel each other when in motion? Evidently mere motion can generate neither attraction nor repulsion. Bodies subjected to a projectile force gravitate with the same intensity and descend, in obedience to terrestrial attraction, through the same perpendicular distance, whether moving with the celerity of a cannon ball, or a planet, or undergoing no impulse excepting those arising from their own unresisted weights.

20. The objections which are thus shown to be applicable in the case of liquids, of which the neighboring particles are destitute of the reaction requisite to produce the phenomena requiring explanation, must operate with still greater force where ethereal fluids are in question, of which the properties are positively irreconcilable with the phenomena. According both to Franklin and Dufay, bodies, when similarly electrified, should repel each other; yet in point of fact, collateral wires, when subjected to similar voltaic discharges, and of course similarly electrified, become reciprocally attractive, while such wires, when dissimilarly electrified by currents which are not analogous, become reciprocally repulsive.

21. Agreeably to Ampère, an iron bar, situated within a coil of wire subjected to a galvanic current, is magnetized, because the current, in the wire, is productive of an electrical whirlpool about every particle of the metal. When the iron is soft, the magnetism, and of course the gyrations of which its magnetism consists by the premises, cease for the most part as soon as the circuit through the coil is broken; but when the iron is in the more rigid state of hardened steel, the gyrations continue for any length of time after the exciting cause has ceased.

22. This theory does not explain wherefore the hardening of the steel should cause the gyration to be more difficult to induce, yet more lasting when its induction is effected. Evidently the metallic particles must take some part in the process; since it is dependent, for its existence and endurance, upon their nature and their state. Yet no function is assigned to these particles. In fact, it is inconceivable, either that they can participate in, or contribute to the supposed gyration.

23. The electric fluid in an iron bar, cannot form a vortex about each particle, all the vortices turning in one direction, without a conflict between those which are contiguous. In order not to conflict with each other, the alternate vortices would

have to turn in different directions, like interlocking cog-wheels in machinery. But in that case, if magnetism be due to currents, the magneto-inductive influence of one set would neutralize that of the other. Again, how can a current, excited by a battery in one circuitous conductor, cause, by dynamic induction, a current in the *opposite direction*, through another conductor parallel to the first, but insulated therefrom? How can a current of quantity in a ribbon coil,* give rise to one of intensity in a coil of fine wire, rushing of course with a velocity commensurate with the intensity thus imparted?

24. From the preceding considerations, and others which will be stated, it follows, that it has been erroneously inferred that the only difference between galvanic and frictional electricity is dependent on quantity and intensity. It must be evident that there is a diversity in the nature of these affections of matter, sufficient to create a line of demarcation between them.

25. Having stated my objections to the electrical theories heretofore advanced, it may be proper that I should suggest any hypothetical views which may appear to me of a character to amend or to supersede those to which I have objected. But however I may have been emboldened to point out defects which have appeared to me to be inherent in the theories heretofore accredited, I am far from presuming to devise any substitute which will be unobjectionable. I am fully aware that there is an obscurity as respects the nature and mutual influence of chemical affinity, heat, light, electricity, magnetism and vitality, which science can only to a minute extent dispel.

26. The hypothesis which I now deem preferable, is so much indebted to the researches and suggestions of Faraday and others, that, were it true, I could claim for myself but a small share of the merit of its origination. That sagacious electrician employs the following language: "*In the long continued course of experimental inquiry, in which I have been engaged, this general result has pressed upon me constantly, namely, the necessity of admitting two forces or directions of force combined with the impossibility of separating these two forces or electricities from each other.*"—*Experimental Researches*, 1163.

27. Subsequently (1244) after citing another proof of the inseparability of the two electric forces, he alleges *it to be* "*another argument in favor of the view that induction and its concomitant phenomena depend upon* a polarity of the particles of matter!"

(*To be continued.*)

* Compendium, Electro-magnetism, paragraph 784.

ART. XXVII.—*An Account of a Compound Achromatic Microscope, made by Charles A. Spencer of Canastota, N. Y.;* by C. R. GILMAN, M.D., Professor of Obstetrics, &c., in the College of Physicians and Surgeons, New York.

THE great importance of the microscope to the cultivators of natural science, and the difficulties and delays which have attended procuring good instruments from Europe, will, I hope, justify me in occupying a small space in this Journal, with an account of a microscope which has recently been constructed for me, by Mr. Charles A. Spencer of Canastota, N. Y., a gentleman, who by force of natural talent, and with no other aid than could be derived from the reading of books on optics and the construction of the microscope, has attained the highest rank as a practical optician.

The microscope is constructed on the model of the smaller microscopes of Charles Chevalier, and like them can be used either in the horizontal, inclined, or vertical position: it can also be inverted so that the stage is directly over the object.

It differs as to the details of its construction from the instruments of Chevalier, in being supported on a tripod like Pritchard's, instead of being screwed into the top of the box, having a safety stage and also a peculiar arrangement by which the object can be moved in azimuth. It has a fine polarizing apparatus and a Lieberkuhn.

There are three objectives of one-third, one-seventh, and one-twelfth inch focus, and two eye-pieces.

With the lower eye-piece, the powers of these combinations are one hundred and twenty-five, three hundred and fifty, and six hundred linear diameters respectively. With the high eye-piece, the one-twelfth inch objective gives a power of eight hundred, and this with an illumination that makes it a fair, available *working* power.

A peculiarity of all the objectives, and one on which their excellence, as Mr. Spencer thinks, mainly depends, is the large angle of aperture; this in the one-seventh inch glass is one-half greater than in the corresponding power of Chevalier, and in the one-twelfth it is nearly double.

Those even moderately familiar with the microscope, will readily appreciate the immense advantage which this wide angle of aperture gives the glasses as a *working* instrument.

The size of the field is great, the three hundred and fifty power has a field more than double that of the three hundred and twenty-five power of Chevalier.

Of the penetrating and defining power, it is perhaps not necessary that I should speak, when the excellence of the instrument

in these respects is vouched for by Prof. Bailey, and by the Report presented to the New York Lyceum of Natural History, by Prof. Torrey, Dr. Le Conte, and Mr. Holton.

It is only because I have been in the daily habit of using the microscope, and by such use have become familiar with its powers, that I venture to add my own opinion, that the glass is for ordinary use in scientific investigations, as superior to the instruments of Chevalier, Plœssels, and Oberhauser, as it has been proved to be when applied to difficult tests. It is both more pleasant and more satisfactory as a working microscope, than any I have seen.

It is with no ordinary satisfaction, that I introduce to the scientific men of the United States, a countryman of our own, who has in the very outset of his career, achieved so great a triumph in practical optics as the construction of a compound achromatic microscope, which compares most favorably with those made by the first artists in Europe. To him we may look for future displays of scientific skill, which shall make us entirely independent of the old world in this important respect.

The friends of American industry and talent will be pleased to hear, that Mr. Spencer has already received orders for microscopes from public institutions and private individuals. Professor Henry has ordered an instrument of high power for the Smithsonian Institute. American patronage, we may hope, will not be wanting to American art.

The testimonial of Prof. Bailey, and the Report alluded to above, are here subjoined.

FROM PROFESSOR BAILEY.

West Point, N. Y., Nov. 27, 1847.

Having carefully examined an Achromatic Microscope made for Dr. Gilman by Charles A. Spencer, Esq., of Canastota, N. Y., I can with confidence as well as pleasure, give my testimonial to its great excellence. I have had it side by side with my own large microscope, (one of Charles Chevalier's largest Achromatics, price 1,000 francs,) and can speak unhesitatingly of their relative merits. I have used Chevalier's instrument with great satisfaction for several years, and have found it in almost every respect convenient and satisfactory. In regard however to the lenses of highest power, it is a fact easily established, by comparative trial, that Spencer's lenses are far superior to any of Chevalier's which I have yet seen. Its superiority consists in a greater angle of aperture and much better definition. With Spencer's glasses I was able, without difficulty, to see the cross lines on the Navicula hippocampus, (the most difficult test object now known to me, and which was sent to me from England, as *the test object par excellence*,) while with Chevalier's, by the same light, I could

see them only with great difficulty. I do not think Spencer's microscope quite equal to the far more expensive English ones made by Ross or Powell, and I cannot positively assert that it is superior to the best of those made by Oberhauser or Plœssels, as I have not had an opportunity to compare them properly side by side in the same light; my impressions however are that the microscope made by Mr. Spencer for Dr. Gilman, is in no respect inferior to any of Oberhauser's that I have seen, and I am by no means certain that it is not superior. I certainly had a better view by *candle light* of the transverse lines of the N. hippocampus, by means of Spencer's instrument, than I was able subsequently to do by *daylight* by means of Oberhauser's. As a proof of the great excellence of Spencer's lenses, I will state that with them, I was not only able to find difficult lines whose existence I was aware of, and for which I was seeking, but on some objects lines of whose existence I had no suspicion, came out so as to force themselves on my attention. I look upon the results obtained by Mr. Spencer, as a proud triumph for American art.

J. W. Bailey,
Prof. Chemistry, U. S. Military Academy.

Copy of the Report on Mr. Charles A. Spencer's Microscope, made to the Lyceum of Natural History.

The committee to whom was referred the microscope of Professor Gilman, having examined the instrument, report as follows:

They deem it not improper to remind even mathematicians not conversant with the use of the microscope, that the goodness of an instrument does not depend on the magnifying power alone. The lower the power sufficient to reveal the structure of a given object, the better the glass, not only as giving a larger field, but also a clearer image. Where a lower magnifying power of one microscope will show the same things as a higher power of another, the former instrument will generally prove the better; the imperfections arising from spherical aberration and dispersion, so far as they remain uncorrected, operate in an increasing ratio with the power used; the liability also to a defective form of lens, is much greater in the higher power. These evils unfortunately will not admit of the remedy applied to lower powers, that of diminishing the aperture, as it would tend to diminish the light and reduce the field of view already too small.

A comparison of microscopes can best be made by using their higher powers, and for a general comparison *similar* test objects placed under each will be sufficient; but for an accurate result, *the same* specimens should be seen by *the same* individual under *similar* circumstances.

Your committee have been able to subject the microscope made by Mr. Charles A. Spencer, to this rigorous comparison with an in-

strument made by Chevalier for one of your committee, and supposed to be one of the best instruments of French make ever imported into this country. The superiority of Mr. Spencer's microscope over Chevalier's number one, *is very decided*, although made on a much smaller and less expensive scale.

JOHN TORREY,
J. F. HOLTON,
JOHN L. LE CONTE, } *Committee.*

ART. XXVIII.—*On Chloroform;* by B. SILLIMAN, JR., A.M.

PUBLIC attention has been much arrested for a few weeks past, by the application which has been made of Chloroform as a substitute for ether, as a means of producing insensibility to pain in surgical cases.

Prof. Simpson of Edinburgh is said to have been the first who made known in this country, this new application of chloroform; although we believe that the medical use of this agent internally, was first recommended by Professor Silliman in his Elements of Chemistry, vol. ii, p. 20, 1830. Some account of chloroform and its preparation, may not be uninteresting at the present time.

It will be remembered that this substance has been long known to chemists, under the name of "oil of the Dutch chemists," and "Dutch oil," from its discovery in 1796, by an association of Dutch chemists at Harlaem, who discovered also at the same time, bicarburetted hydrogen gas, which they named olefiant gas, from its producing this oily compound when mingled with its own volume of chlorine. The terms chloric ether, bichloric ether, perchlorid of formyle, Dutch oil, and oil of the Dutch chemists, are all synonyms of chloroform, a term first proposed by Dumas, signifying, as he suggested, the relation of this substance to formic acid, or its hypothetical radical formyle.

The production of "chloric ether" so called, by the action of alcohol with bleaching powders, was discovered almost simultaneously and without conference by our ingenious countryman, Samuel Guthrie, of Sackett's Harbor, New York,* and by M. E. Soubeiran† in France. In both cases alcohol was distilled from a solution of bleaching powders in water, but in varying proportions. For the particulars of their processes, the reader is referred to the volumes quoted below. The medicinal article known

* See this Journal, i ser., xxi, 64, xxii, 105, January, 1832.

† Annales de Chim. et de Phys., October, 1831, quoted in this Journal, i ser., xxiii, 134. Other references to this subject are—vol. xxi, 405, and xxii, 163.

under the name of "chloric ether," as it is usually found in trade, is a very impure alcoholic mixture, which retains very few of the properties of chloroform, and is probably of no value in medicine, the characters of the alcohol being altogether dominant.

The following process for producing chloroform, has been found most efficacious in the large way. About six pounds of dry bleaching powders are introduced into a copper still containing about four gallons of water, and having a capacity of ten or twelve gallons. Half a gallon of common alcohol (about eighty per cent.) is added—the materials are thoroughly mingled by agitation, and a cautious distillation commenced. A powerful condensing apparatus is attached, and very soon the dense globules of chloroform are seen falling into the receiver. The larger portion of the product is given off before the fluid has become hot enough to distill over much alcohol. Nevertheless it will be found desirable to change the receiving vessel when the process is judged to be half done, as the first portions are by far the richest, and if allowed to mingle with the more alcoholic part which comes over last, the entire product of the distillation is much smaller, because a very large portion of chloroform is capable of being dissolved in a very small one of alcohol. The product of the distillation is agitated with twice or more its own volume of cold water, in a tall vessel of glass, when the chloroform is seen to collect rapidly on standing, in a dense stratum beneath the water. The two fluids are separated by a funnel and the finger, and the washing is repeated until the chloroform suffers no further perceptible decrease of volume. The distillation is continued until water ceases to precipitate the chloroform from its alcoholic solution, and afterwards until so much of the excess of alcohol is removed as is judged best by the operator. The washings of this process, as well as the dilute alcohol, the product of the last stages of the distillation, are all preserved for a second charge. The "caput mortuum" in the still I have found to contain a considerable portion of chlorine, but not sufficient to produce any more chloroform by the addition of more alcohol; and the quantity of alcohol above recommended cannot be much decreased, (although a decided excess is employed,) without contaminating the product with free chlorine, which especially for the purposes of inhalation, is very objectionable, and is not easily removed by the washing. The solution of chlorid of lime and alcohol remaining after distillation, deposits on standing, an abundant crop of delicate silky acicular crystals, whose constitution has not been examined. The proportions above named, with care yield six to eight ounces of pure chloroform.

Chloroform prepared as above directed, has a specific gravity of 1·48, and I have found no advantage in redistilling it from sul-

phuric acid, to deprive it of water or to increase its density. Some care and experience are required to wash it economically. Thus prepared it is an oily, highly volatile and fragrant fluid, perfectly pellucid, with a sweet and pungent aromatic taste. Its vapor mingled with atmospheric air is highly agreeable, and produces at once the symptoms of intoxication. It boils at 141° F., and is not inflammable, but its vapor, as observed by M. Soubeiran, if directed from a jet upon the flame of a spirit lamp, takes fire and burns with much smoke. It also burns when mingled with an equal volume of alcohol. Iodine dissolves in it. Potassium decomposes it slowly at common temperatures, disengaging hydrogen and some carbon, and forming chlorid of potassium. Barytes and lime also decompose it, with the production of a metallic chlorid, and the separation of free carbon and inflammable gas. It is not decomposed by sulphuric acid, and hydrochloric acid does not change it even with heat. Heated with nitric acid, a small quantity of nitrous vapors are exhaled. A concentrated solution of potash produces a slow decomposition of the compound.

The constitution of chloroform is best expressed by the formula C_2HCl_3, or it is formene C_2H_4, in which three equivalents of hydrogen are *replaced* by three of chlorine, without destroying the type of the original compound. Formene is the name which has been given to marsh gas, and has the same constitution as wood-spirit minus two atoms of oxygen. $C_2H_4O_2 - O_2 = C_2H_4$. Although the chloroform contains more than eighty-three per cent. of chlorine, this element cannot be detected in it by nitrate of silver. This apparent anomaly is in obedience to the law which governs all similar substitution products, in which the electro-negative element exists in an electro-positive allotropic condition. Not the slightest analogy can be traced between the constitution of chloroform and the ethers, and hence the great impropriety of calling it "chloric ether," which term belongs to the perchloric ether described by Booth and Boyé, a totally dissimilar compound, of much explosive energy, which is $C_4H_5ClO_8$. If chloroform were an ether, it ought in analogy with all other compounds of that family, promptly to be decomposed by watery solution of potash, taking up two equivalents of oxygen, and regenerating alcohol and chlorine, which is obviously not the fact and quite impossible. Iodoform and bromoform are two compounds (C_2HI_3 and C_2HBr_3) in strict analogy with chloroform. All these compounds are decomposed by an *alcoholic solution of potash*, affording a compound of the salt radical with potassium, and formate of potash. Thus $C_2HCl_3 + 4KO = 3KCl + C_2HKO_4$. Trichlorinized formene is a name which exactly describes the composition of chloroform, and the fourth equivalent of hydrogen which it contains, may be replaced by exposing chloroform to the action

of chlorine gas in the sun's light; and chlorid of carbon produced C_2Cl_4, or perchlorinized formene.*

In the employment of chloroform as an agent to produce insensibility, too little experience has been had to enable us to form a just comparison between it and the common sulphuric ether before employed for the same purpose. It appears however thus far, that a much smaller portion of chloroform is required to produce the same or even greater effects. Thirty drops on a sponge or handkerchief are said to suffice in most cases, but we have seen other cases in which ten times that quantity was required. The insensibility and consequent relaxation of the patient are perfect, and apparently much more prolonged than in etherization. The inhalation is far more agreeable to the patient, and not so often attended with paroxysmal efforts at coughing. Further and extended experience is however required before its true value can be fully known. Meanwhile the zeal for manufacturing it will make it a comparatively cheap article, as there is nothing in its production requiring great expensiveness.

Analytical Laboratory, Yale College, Jan. 15, 1848.

Art. XXIX.—*Proceedings of the American Association of Geologists and Naturalists at Boston, September*, 1847.

I. *Upon some of the Results of the Palæontological Investigations in the State of New York.*†

Mr. Hall commenced by stating that this work was committed to his charge in the year 1843. At that time it was intended to embrace the fossils of the entire series in a single volume;

* The December number of the Ann. de Chim. et de Phys., p. 439, contains an article by M. J. Isidore Pierre, "on a new chlorine derivative of Dutch liquid." (*Sur un nouveau dérivé chloré de la liqueur des Hollandais.*) This new compound he represents by the formula $C_4\,HCl_5$. M. Pierre takes a different view of the constitution of Dutch liquid from that presented in the text, representing it (on the single equivalent of hydrogen) as $C_4\,H_4\,Cl_2$; the chlorine series given by him, is,

$C_4\,H_4\,Cl_2$
$C_4\,H_3\,Cl_3$
$C_4\,H_2\,Cl_4$
$C_4\,H\,Cl_5$

A series of which each term, under the influence of an alcoholic solution of potassa, loses the elements of 1 equivalent of chlorohydric acid, giving the series,

$C_4\,H_3\,Cl$
$C_4\,H_2\,Cl_2$
$C_4\,H\,Cl_3$
$C_4\,Cl_4$ of which olefiant gas is regarded as the fundamental type. The new member of this series is the product of the reaction of a rapid current of chlorine upon Dutch liquid, under the influence of the sun's rays. Repeated distillation gives a fluid which boils at 153°·8 and with a sp. gr. of 1·662, with a less agreeable and sweet taste than Dutch liquid.

† The completion of the 1st volume of Mr. Hall's Palæontological Report was announced in our last Number, page 149, of this volume. The second volume is making rapid progress.—Eds.

but after working two years it became apparent, from the accumulation of materials, that a change of plan must be adopted; and from that time more particular attention had been devoted to the lower division of the system, or to that part of the series terminating with the Hudson River group of New York.

During all this time there had been a constant accumulation of species apparently new, and the desire to present all these as far as possible in the work, had caused some delay. He remarked, that it was not for the sake of presenting a great number of new species, that he had been desirous to give all these, but that the lower term of the system should be as fairly and equally represented as the middle and higher portions which had been more thoroughly studied, and from which a greater proportional number of species had been heretofore described. When the present work was commenced, less than seventy species had been described from that portion of the system in the United States, and it could scarcely have been expected that this number could have been more than doubled within the limited area of a single state; and yet there are at this time already described, 380 species, most of which are from New York. Beyond those described, there are others, in an imperfect and unsatisfactory condition, sufficient to swell this number to more than four hundred.

Mr. Hall remarked, that comparing these results with those of the great pioneer in the labor of investigating the palæozoic strata, Sir R. I. Murchison, he flattered himself that some little progress had been made, since we were able to show from our strata, equivalents of the lower Silurian of Europe, a greater number of species than were enumerated in the "Silurian system," at the time of its publication in 1839.

Another, and to the geologist a more important point, after the determination of species, was to ascertain accurately their geological range. Examinations of species, supposed to be identical, from widely separated groups, had always proved them unlike; and many forms regarded as belonging to the same species, were only so regarded for want of sufficient examination. Continued investigations, instead of showing a greater number of species common to two or more widely separated groups of the older strata, had only shown us more accurate means of discrimination, and had enabled us to prove that a very small number only had lived on from one period to another. If certain species of fossils were to be regarded as guides in determining the age of strata, it was necessary that these species should not be confounded with other similar ones, which may have come into existence at a previous or subsequent period. For although we can assign no reason why a considerable part of the species of a group, or perhaps all, may not have continued their existence while a few have done so, and while the exterminated ones have been replaced by

others of similar type, yet our daily experience proves to us the truth of this principle as shown in the organized remains of successive strata.

Mr. Hall remarked that, when a few years since he maintained that the single genus *Catenipora* would be found typical of certain strata, and a reliable guide from New York far to the west of the Mississippi, the idea was regarded as absurd and was absolutely ridiculed. Now he was happy to say to the Association, that other observers than himself had volunteered to sustain that opinion, and the *Catenipora* was regarded not only here but in Europe, as marking unequivocally a certain stage in the system, never appearing in the lower Silurian or rising into the Devonian. Both M. de Verneuil and M. Rœmer had confirmed this opinion by extensive observations in the United States. If then, a single species of coral is so limited in its geological range, what may we not expect from other remains of higher organization?

He remarked that the *Pentamerus oblongus* likewise marks a certain and never varying horizon, over more than a thousand miles in extent, and the same remark might be applied to other species more or less conspicuous in every part of the series.

As an example of a fossil supposed to have a wide geological range, he instanced the *Favosites lycoperdon* of Say,—*Chætetes petropolitanus* of Pander,—which had been pointed out to him as occurring in the lower stage of the Silurian period, and in the higher Devonian. Some of these from the central portions of the series on being examined, showed very clearly the small connecting foramina, which never appear in the similar forms from the lower strata. These fossils although so similar in appearance, prove to belong to distinct genera. There are however, he remarked, examples where the fossils of distinct groups simulate each other so nearly, as to render it difficult to distinguish them from each other. The most striking instance of this, is between the Niagara group and a part of the succeeding formation designated as the Delthyris shaly limestone. Several species of the Brachiopoda of the former, are represented by others in the latter, so nearly like them that they are often regarded as identical. The examples of these are seen in *Delthyris Niagarensis*, which are represented by *D. macropleura*—*D. crispus*, which is represented by *D. micerptera* of Conrad—and *D. octoplicata*, which is represented by a larger species with similar marking. The small *D. biloba* of the Niagara group, is represented by a similar peculiar form in the shaly limestone, which is nevertheless distinct.* *Orthis elegantula* and *O. hybrida* are represented by similar forms,

* M. de Verneuil regards the D. biloba of the Wenlock formation of Europe, as identical with that from the Delthyris shaly limestone of the Helderberg. This may be true, but our Niagara species appears to us to be quite distinct.

which are usually regarded as identical, but which are entirely distinct.

Mr. H. said he did not intend to go into detail in this place, with remarks of this kind, it being his intention merely to present the results of his investigations in the strata constituting the lower term of the system; these had been arranged in tabular form, showing the number of genera and species of each class or order, with their distribution, and range beyond the point of their first appearance. In this table nothing had been supposed, every thing presented having been verified from observations, and the slightest evidence, such as the occurrence of the fragment of a fossil in a higher position, had been seized upon to show the greater range of the species.

Table showing the number of species peculiar to each Rock and Group, and also the number common to several groups.

CLASS OR ORDER.	GENERA.	SPECIES.	RESTRICTED TO THE									COMMON TO THE								
			Potsdam sandstone.	Calciferous sandstone.	Chazy limestone.	Birdseye limestone.	Black River limestone.	Trenton limestone.	Utica slate.	Hudson River group.		Potsdam and succeeding strata.	Calciferous and succeeding strata.	Chazy and Black River limestones.	Birdseye, Black River and Trenton limestones, & Hudson River group	Birdseye and Black River limestones	Black River and Trenton limestones.	Trenton, U. slate & Hudson River.	Trenton limestone and Utica slate.	Trenton and Hudson River group.	Utica slate and Hudson River group.
Plantæ,	4	14	1	3	·	·	·	4	1	5		·	·	·	·	·	·	·	·	1	·
Incertæ sedes,	3	4	·	·	·	2	·	·	·	1		·	·	·	·	·	·	·	·	1	·
Zoophyta,	19	50	·	·	7	1	3	19	3	13		·	·	·	1	1	1?	·	·	1	·
Crinoidea,	8	15	·	·	3	·	·	7	·	3		·	·	·	·	·	·	·	·	1+1?	·
Brachiopoda,	7	77	2	1	10	·	·	51	·	5		·	·	·	·	·	·	3	1	3+1?	·
Acephala,	12	49	·	·	1	1	·	26	1	13		·	·	·	·	·	·	·	·	4+1?	2?
Gasteropoda,	17	71	·	8	13	9	·	28	·	6		·	·	1	·	·	1	·	·	4+1?	·
Cephalopoda,	11	68	·	1	4	2	10	40	2	5		·	1?	·	·	·	1?	·	1	1?	·
Crustacea,	14	33	·	·	7	4	·	13	1	3		·	·	·	·	·	·	3	·	1	1?
Total,	95	381	3	13	45	19	13	188	8	54		0	1	1	1	1	3	6	2	20	3

The proportion of species in the different orders of Mollusca, with the exception of Acephala, is nearly the same.* Notwithstanding that care had been taken to present all the well marked species of this order, and the proportional number had been greatly increased beyond what any previous publication had shown, still there is a manifest deficiency in shells of this order during the early periods, as compared with either the Brachiopoda, Gasteropoda or Cephalopoda. By uniting all the radiated

* Among the Brachiopods, a few western species have been introduced, while in the other orders nearly every species is known in New York; the slight excess in this order is due to embracing a wider geographical range.

forms including the Bryozoa, we have a number of species equal to either of the orders of Mollusca, while the number of crustaceans known is not more than half as great. Farther researches, however, in a more extended field of observation, and where the nature of the sediment has influenced the character of the fauna, may produce species which will change the proportions here given.

These researches had shown also that there were not only species peculiar to the lower stage of the system, but that generic forms were likewise characteristic, and that certain of these assume, as it were, a gradual development or change in ascending the geological scale. It was therefore not always necessary to search for numerous species, to determine the geological horizon, since one or a few generic forms or *chronotypes* are all that is sufficient for this purpose.

Among the corals, there are no true Favosites known in lower Silurian strata, all the allied forms having no lateral or connecting foramina, and are distinguished as Chætetes. In those corals having the turbinate forms of Cyathophyllum, there are no species having transverse dissepiments, the vertical lamellæ alone constituting the interior structure, having a slight contortion when they meet in the centre. Even in the central portion of the system, it is doubtful if there are any true Cyathophylla, the genus becoming fully developed only in the Devonian period. Another eminent feature of the lower Silurian stage, is the almost entire absence of the Reteporoid forms of Zoophytes, which in the next stage assume an important place, which they continue to hold throughout the Devonian period. Among the Crinoidea, we find several generic forms peculiar to this period, and all those known present characters distinguishing them from those of succeeding stages.

In the Brachiopoda, although there is always an apparent preponderance of *Orthis* and *Leptæna*, a careful examination shows that the number of species of these two genera and of *Atrypa*, present scarcely a difference in number of species, while the *Spirifer* or *Delthyris* is represented in a single species only. The latter genus, though often numerous in the second stage of the system, is only found in its maximum development in the Devonian period, while the species of *Orthis* and *Leptæna* show a constant diminution in number as we ascend in the scale of formations. The Acephala on the other hand, are comparatively numerous in the lower stage of the system, while they are rare in the second, presenting however a remarkable development in the upper part of the Devonian system. The single genus *Avicula*, which first appears in the lower stage, reappears in the succeeding one, becomes numerous in the Devonian period, and like the *Lingula* among the Brachiopods, preserves its integrity throughout, and after living in every geological period from the

earliest to the most recent, present themselves as living forms in the present ocean.

Among the Gasteropoda, we find several generic forms which thus far have not been recognized in the higher strata. Among these are *Maclurea*, *Scalites*, *Bucania*, *Cystolites* and others, which are common, and some of them abundant forms in the lower period. The genus *Murchisonia* likewise appears, with us, to belong almost exclusively to the lower Silurian period, few or no species having been recognized in a higher position. The genus *Pleurotomaria*, on the other hand, appears throughout the Palæozoic strata.

The genus *Acroculia* does not appear in the lower Silurian strata, and is but faintly represented in the lower stages of the upper Silurian, while in the higher strata of the same division, it is more abundant than any other univalve. It is likewise continued into the higher or Devonian strata, being more abundant near the commencement of this period, and dying out towards the termination of the same.

We recognize among the Cephalopoda the genera *Gonioceras*, *Eudoceras* and *Oncoceras*, which possess peculiar characters unknown in shells of this order in the subsequent stages of the system, while *Ormoceras* seems to mark even a more limited period of the lower stage of the system. The genus *Litnites*, which marks the lower divisions of the system, is scarcely represented in the second and third stages, even by any analogous form, while *Cyrtoceras*, and afterwards *Goniatites*, follow in the Devonian period in much greater numbers, both of species and individuals, than we find of the former genus in the lower term of the system.

Almost all the well established genera of American Trilobites, appear in the lower term of the system. The genera *Isotelus* and *Illænus*, with *Trinucleus*, are the prevailing and typical forms, which are unknown in any higher position. The genus *Ceraurus* seems likewise typical of strata of the lower Silurian period, as well as the genus *Olenus*, of which we have some imperfect specimens.

The genera *Calymene*, *Phacops*, *Asaphus*, *Platynotus* or *Lichas*, are known in the subsequent stages of the system. The genus *Calymene* is no where else so abundant as in the lower term of the system, though scarcely less so in the middle, or lower stage of the upper Silurian, while it is scarcely known above the base of the Devonian system. The genus *Phacops*, which is but poorly represented in the lower term of the system, is more abundant in the middle, and one species, the *P. macrophthalma*, is rare in the Silurian period, but becomes abundant in the Devonian. The genus *Asaphus* appears near the base of the upper Silurian, but is unknown to us in the Devonian. The genus *Lichas*, (*Platynotus*, Conrad,) is represented by a single species in

the lower Silurian period, by another in the lower stage of the upper Silurian, and by two or more species in the higher strata of this period. It is however unknown in the Devonian period.

The exhibition of the geological range of species as shown in the preceding table, proves that the lower stage of this division of the system is susceptible of being separated from the upper, being almost disconnected by its fossil contents. Of the one hundred species in the strata below the Trenton period, only seven are known to ascend into that rock and the succeeding strata. Still there is no strong line of demarkation, and there often appears to be a mingling of the materials of the two portions at the point of junction, as well as of the organic forms.

The larger number of species of the Trenton limestone period, which pass upwards into the succeeding rocks, notwithstanding their dissimilarity of composition, show a very intimate relation, proving that but a single period is indicated by the fauna. This is further proved by examinations farther westward, where the distinctions of Trenton limestone, Utica slate, and Hudson River group, are not recognizable, the entire composition being calcareous.

At the termination of this period to which more particular allusion is now made, we find a strong lithological line of demarkation. The number of species which passes beyond this limit is exceedingly few, and their appearance in a subsequent period is, for the most part, so rare that they give no characteristic feature to the strata, nor are they ever likely to produce any confusion in their identification. We have not, as is said to be the fact in Europe, a mingling of the fossil contents of the upper and lower Silurian periods, at the junction of the strata of these two epochs. On the other hand, we are able to demonstrate over a considerable portion of country, an entire lithological change at this point, and an examination of the organic remains proves these evidences not less unequivocal than the others.

II. *Prof. Gale on the Natchez Bluff Formation.*—This paper consists chiefly of an inquiry respecting the geological character of *fossil human bones*, announced by Dr. M. W. Dickerson as discovered by him in the above formation, and so reported to the Association at their sittings in 1846. The report above referred to stated that the fragment of bone was discovered in the same formation, and almost in juxtaposition, with the cranium of the *Megalonyx laqueatus* of Harlan. So extraordinary and important a fact, if such it should prove to be, led the writer to investigate the circumstances connected with the discovery, and the report presented purports to be the result of the investigation.

It embraces a description of the geology of the Natchez bluff formation, showing that the portion containing the fossils in ques-

tion, is the most recent of the tertiary beds found in this section of the country: that its fossils are of land or freshwater origin: that the facts developed admit of the possibility that the human bone as well as those of the Megalonyx, may have found their way to their recent resting place within the historic era; but as the former is known to be extinct, genus and species, and the latter hitherto confined to the existing race, we have no occasion from the facts here investigated, to modify the views hitherto entertained as to the history of our species.

ART. XXX.—*Abstract of a Meteorological Journal for the year* 1847, *kept at Marietta, Ohio,* Lat. 39° 25′ N., and Long. 4° 28′ W. of Washington City; by S. P. HILDRETH, M.D.

MONTHS.	THERMOMETER.			Fair days.	Cloudy days.	Rain and melted snow.		Prevailing winds.	BAROMETER.		
	Mean temperature.	Maximum.	Minimum.			Inches.	100ths		Maximum.	Minimum.	Range.
January, - -	31·76	69	−2	17	14	3	66	N. & S. W.	29·88	28·95	0·93
February, -	35·66	66	8	8	20	2	88	N. & N. W.	29·68	28·95	0·73
March, - -	39·70	73	15	15	16	4	30	S. & S. W.	29·85	29·05	0·80
April, - -	53·83	82	24	24	6	2	54	S. W. & N.	29·70	29·25	0·45
May, - -	61·90	84	35	18	13	1	75	S. W. & S.	29·70	29·20	0·50
June, - -	68·63	85	46	22	8	2	77	S. & S. E.	29·55	29·23	0·32
July, - -	71·83	90	48	20	11	6	80	E. S. E. & S.	29·60	29·28	0·32
August, - -	67·80	85	50	17	14	2	80	S. E. & S.	29·60	29·25	0·35
September, -	62·13	86	40	18	12	4	84	S. S. E. & N.	29·65	29·20	0·45
October, - -	50·80	74	22	19	12	6	42	W. & N.	29·95	29·05	0·90
November, -	44·83	76	14	16	20	2	70	S., N. & W.	29·85	29·15	0.70
December, -	35·33	69	2	10	21	10	84	S., N. E. & N.	29·70	29·05	0·65
Mean for year,	52·00				52	30					

Remarks on the year 1847.—The mean temperature for the year is 52°; which is lower by a degree and a half than in 1846; that is, about the average mean for this locality. The mean for the winter months is 35°; for the spring, 51°·81; summer, 69°·42; and autumn, 52°·25.

The year which has just closed, has not been remarkable for any great deviations from the usual course, except the excessive rains in December, causing floods in all the western rivers, and which will be more especially noticed when speaking of the autumn. The crops of grain were of an average amount in quantity and quality. That calamitous disease, the "potato rot," did not assail many of our fields, and only in places where the ground was unusually wet, at a time of excessive heat in June and July, with a scalding sunshine. Under such circumstances whole fields

were destroyed in a few hours. The crops of Indian corn were late in ripening, owing to the cool weather in August and September, which two months combined, were fourteen degrees lower in temperature than those of 1846. This important grain, however, finally matured very well, escaping the action of severe frosts. The crops of apples and peaches were nearly all destroyed by a frost on the 18th of April, when the latter were in full bloom, and the former just ready to open. As this was not the regular bearing year for apples, they were more easily killed. The crop of grapes was much injured by the excessive rains of July, causing a mildew on the leaves, and the rotting of the fruit. This desirable fruit is now considerably cultivated in this vicinity, especially the more hardy varieties. Horticulture in its various aspects has many votaries among us; and within a few years, a great change has taken place in the taste of the inhabitants, for the ornamental as well as the really useful. All these ameliorations refine the heart and the manners, and are new sources of contentment and happiness.

The amount of rain and melted snow is 52·30 inches; which is six inches more than in 1846; and even that year was several inches above the mean quantity. Twenty-five inches of rain fell in the four last months of the year; of which nearly eleven fell in December, causing the extensive floods in all the western rivers, more general than at any former historical period.

The mean temperature of the winter months is about five degrees higher than the last, the Ohio River remaining open for navigation. The greatest cold was two degrees below zero, on the 12th of January, when the ground was covered with five inches of snow. It remained cold for a week, causing considerable floating ice, but did not stop the running of steamboats. Very little ice was laid up for summer use, none being made over three or four inches in thickness. The remainder of the winter was mild and pleasant.

The mean temperature of the spring months was 51°·81, which is below that of the preceding year. April was nearly three degrees warmer than in 1846. This season however was mild and pleasant, not marked by any unusual deviation from its ordinary course. Frosts more or less intense are expected when the peach and apple are in blossom along the immediate vicinity of the Ohio River, and on the 18th of April a visitation of this kind destroyed nearly all the fruit in this and the adjacent river counties. On the higher grounds some miles back, much fruit escaped.

Floral calendar for 1847, *migration of birds, &c.*—February 20th, blue-bird heard; 21st, honey-bee at work; 22d, robin appears.

March 6th, Crocus in bloom. The rest of the month was cold and stormy, retarding vegetation. 29th, Triloba hepatica in bloom, usually out the fore part of this month, as early as the garden Crocus, in great variety and beauty; 30th, early hyacinth.

April 1st, Acer saccharinum in bloom; 5th, apricot; 6th, cotton-wood; 7th, crown imperial; 8th, peach, in warm exposures, is opening its petals, Pyrus japonicus, mirobolans plum; 9th, primroses; 10th, dwarf iris, Sanguinaria canadensis; 11th, peach in full bloom, also June berry; 12th, imperial gage; 18th, severe frost, killing fruit blossoms; 21st, pear in bloom, Arkansas plum; 23d, cherry; 24th, harebell; 25th, red-bud or Judas tree; 29th, Cornus florida, tulips.

May 3d, Cornus florida in perfection; 4th, smart frost, apple shedding its blossoms; 5th, papaw; 11th, Magnolia tripetola; 12th, black haw; 13th, yellow moccasin flower. The pink colored variety in my garden, from near Galena, in the north of Illinois, does not bloom until the last of the month or first of June, preserving its more northerly habit, although a resident here of eight or ten years. 15th, yellow Harrison rose, white Scotch, &c.; 23d, locust, Robinia acacia; 26th, Ribes villosa; 27th, black walnut.

June 2d, rose-bug, melolantha sub-spinosa appears in great multitudes on roses, &c., but congregates in the largest numbers on a pecan tree; 3d, Osage orange in bloom; 8th, persimmon; 12th, white lily; 14th, cucumbers grown in the open air fit for eating; 17th, yellow Antwerp raspberry ripe; 22d, Catalpa in bloom.

Summer.—The mean of the summer months is 69°·42, which is a degree and a half below that of 1846. The whole of this season was very cool and wet, retarding the ripening of melons, Indian corn, sweet potatoes, and many other articles beyond the usual period. July was excessively wet, there falling near seven inches of rain, and making it difficult to cure the crops of hay, and save the shocks of wheat from vegetating while standing in the field; considerable grain was lost from this cause, but it was free from rust and mildew.

Autumn.—The mean temperature of the autumnal months is 52°·55, nearly four degrees below that of the preceding year. The greatest difference was found in September, being a variance of seven degrees from 1846. October and November were very rainy months, occasioning a flood in the Great Kenawha, which did much damage to the salt manufactories along its margin in November, a prelude to the great flood in December following. In this month there were 10·84 inches of rain, nearly nine of them in about five days, or from the 9th to the 14th of the month, three and a half inches falling on the night of the 10th, and two and a half on the night of the 13th, with daily intermediate rains. On the 10th the Muskingum rose fifteen feet in a few hours, and

by the 11th, both rivers had risen twenty feet. On the 13th, the water was over all the banks, and by the 14th was at its height, lacking only five feet of the great flood of 1832, and was only prevented from an equal elevation by the smaller amount of rain on the heads of the Monongahela and Alleghany Rivers, which streams were not unusually high. It is singular that the floods of 1813 and 1815 were both of the same elevation as that of 1847, differing only a few inches. On the night of the 14th December, there fell four inches of snow at Marietta, and eighteen inches at Cincinnati, where the water was within a foot of the rise of 1832. The Muskingum was as high at Zanesville as in the latter year, and did far more damage, sweeping the bottom lands of the crops of corn, from its heads to the mouth, as a large portion of this valuable grain was yet standing in shocks on the ground where it grew. For twenty-four hours the surface of the river was covered with floating stacks of grain, hay and corn, with now and then a log cabin and masses of fencing stuff. Mr. Dana of Waterford, twenty miles above Marietta, lost one thousand sheep, nearly all ewes, of the finest Merino and Saxony blood, drowned in the waters which overflowed the bottoms in the night. Immense damage was done to the slack water improvement of the Muskingum, by the flood cutting channels round several of the dams, forming a new bed for the river. At Pittsburgh and Wheeling the water was fifteen or twenty feet below that of 1832, but on all the borders below the mouth of the Muskingum, the loss was incalculable to the farmers, sweeping away their crops of grain and hay, with all their fences, leaving them destitute of food for their cattle and horses, which can only be saved from starvation by driving them back into the hill region, beyond the reach of the flood, where there is an abundance for all the stock. At the mouth of the Scioto River, the water was as high as in 1832, the borders of that stream being flooded from its head to its junction with the Ohio, destroying thousands of cattle and hogs, as well as the crops. In its effects, the flood of 1847 was more disastrous than any former one, as it came at so early a period in the winter, before the crops could be applied to the use of man or beast. Several millions of dollars would not make good the loss to the country. Mills and bridges without number were swept away, while the roads are left impassable from the depth of mud, opening a winter of great distress to the dwellers in the valleys of our western streams. As the bottom lands become more cleared of the trees, which clothe them like a garment, it is probable that the rise of water in the rivers will be somewhat diminished; at least one-eighth of the surface is occupied by the trunks of the trees, and when removed, it will take more water to cover the bottoms.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Diamagnetic Conditions of Flame and Gases;* by Michael Faraday, F.R.S., (Phil. Mag., No. 210.)—Faraday alludes in the first place to the discovery of M. Bancalari on the magnetism of flame, and the recent researches on this subject by Zantedeschi. He then proceeds with the details of his own experiments, by which he has confirmed these results.

"The electro-magnet used was the powerful one described in the Experimental Researches, 2247. The two terminal pieces of iron forming the virtual magnetic poles, were each 1·7 inch square and six inches long; but the ends were shaped to a form approaching that of a cone, of which the sides have an angle of about 100°, and the axis of which is horizontal and in the upper surface of the pieces of iron. The apex of each end was rounded; nearly a tenth of an inch of the cone being in this way removed. When these terminations are brought near to each other, they give a powerful effect in the magnetic field, and the axial line of magnetic force is of course horizontal, and on a level nearly with the upper surface of the bars. I have found this form exceedingly advantageous in a great variety of experiments.

"When the flame of a wax taper was held near the axial line, but on one side or the other, about one-third of the flame rising above the level of the upper surface of the poles, as soon as the magnetic force was on, the flame was affected, and receded from the axial line, moving equatorially, until it took an inclined position, as if a gentle wind was causing its deflection from the upright position, an effect which ceased the instant the magnetism was removed.

"The effect was not instantaneous, but rose gradually to a maximum. It ceased very quickly when the magnetism was removed. The progressive increase is due to the gradual production of currents in the air about the magnetic field, which tend to be, and are, formed on the assumption of the magnetic conditions, in the presence of the flame.

"When the flame was placed so as to rise truly across the magnetic axis, the effect of the magnetism was to compress the flame between the points of the poles, making it recede in the direction of the axial line from the poles towards the middle transverse plane, and also to shorten the top of the flame. At the same time the top and sides of the compressed part burnt more vividly, because of two streams of air which set in from the poles on each side directly against the flame, and then passed out with it in the equatorial direction. But there was at the same time a repulsion or recession of the parts of the flame from the axial line; for those portions which were below did not ascend so quickly as before, and in ascending they also passed off in an inclined and equatorial direction.

"On raising the flame a little more, the effect of the magnetic force was to increase the intensity of the results just described, and the flame actually became of a fish-tail shape, disposed across the magnetic axis.

"If the flame was raised until about two-thirds of it were above the level of the axial line, and the poles approached so near to each other (about 0·3 of an inch) that they began to cool and compress the part of the flame at the axial line, yet without interfering with its rising freely between them; then, on rendering the magnet active, the flame became more and more compressed and shortened; and as the effects proceeded to a maximum, the top at last descended, and the flame no more rose between the magnetic poles, but spread out right and left on each side of the axial line, producing a double flame with two long tongues. This flame was very bright along the upper extended forked edge, being there invigorated by a current of air which *descended* from between the poles on to the flame at this part, and in fact drove it away in the equatorial direction.

"When the magnet was thrown out of action, the flame resumed its ordinary upright form between the poles, at once; being depressed and redivided again by the renewal of the magnetic action.

"When a small flame, only about one-third of an inch high, was placed between the poles, the magnetic force instantly flattened it into an equatorial disc.

"If a ball of cotton about the size of a nut be bound up by wire, soaked in ether and inflamed, it will give a flame six or seven inches high. This flame rises freely and naturally between the poles; but as soon as the magnet is rendered active, it divides and passes off in two flames, the one on one side, and the other on the other side of the axial line.

"Such therefore is the general and very striking effect which may be produced on a flame by magnetic action, the important discovery of which we owe to P. Bancalari."

Faraday verified these results with different flames. He also from his experiments shows that hot air and gases generally are *more diamagnetic* than cold air. His experiments on the magnetic character of gases, were performed by "delivering streams of each into the air, in the first instance, by fit apparatus and arrangements, and examining the course taken by these streams in passing across the magnetic field, the magnetic force being either induced or not at the time." To detect the course of the streams, catch-tubes of thin glass were arranged, usually one over (or below, if the gas was lighter than the atmosphere) the axial line, and one at each side. These tubes were open at both ends. The air was kept undisturbed about the magnetic poles by a "sheltering chamber" of mica. Of the trial with nitrogen, oxygen and hydrogen, Faraday remarks:

"*Nitrogen.*—This gas was sent from below upwards, and passed directly by the axial line into the catch-tube above; but when the magnet was made active, the stream was affected, and though not stopped in the middle catch-tube, part appeared in the side tubes. The jet was then arranged a little on one side of the axial line, so that, without the magnetic action, it still ascended and went up the middle catch-tube: then, when the magnetic action was brought on, it was clearly affected, and a great portion of it was sent to the side catch-tube. The nitrogen was, in fact, manifestly diamagnetic in relation to common air, when both were at the same temperature; but as four-fifths of the atmosphere

consist of nitrogen, it seemed very evident, from the result, that nitrogen and oxygen must be very different from each other in their magnetic relations.

"*Oxygen.*—A stream of oxygen was sent down through air between the poles. When there was no magnetic action it descended vertically, and when the magnetic action was on, it appeared to do the same; at all events, it did not pass off equatorially. But as there was reason, from the above experiments with nitrogen, to expect that oxygen would appear, not diamagnetic but magnetic in air; so the place of the stream was changed and made to be on one side of the axial line. In this case it fell perfectly well at first into a catch-tube placed beneath; but as soon as the magnet was rendered active, the stream was deflected, being drawn towards the axial line, and fell into another catch-tube placed there to receive it. So oxygen appears to be magnetic in common air. Whether it be really so, or only less diamagnetic than air (a mixture of oxygen and nitrogen), we shall be better able to consider hereafter.

"*Hydrogen.*—This gas proved to be clearly and even strongly diamagnetic; for notwithstanding the powerful ascensive force which its stream has in the atmosphere, because of its small specific gravity, still it was well deflected and sent equatorially. Considering the lightness of the gas, one might have expected that it would have been drawn towards the axial line, as a stream of rarefied air (if it could exist) would be. Its diamagnetic state, therefore, shows in a striking point of view, that gases, like solids, have peculiar and distinctive degrees of diamagnetic force."

He afterwards made trials with carbonic acid, carbonic oxyd, nitrous oxyd, nitric oxyd, nitrous acid gas, olefiant gas, coal gas, sulphurous acid gas, muriatic acid, hydriodic acid, ammonia, fluosilicon, chlorine, iodine, bromine and cyanogen, and found that they were all diamagnetic in air.

The relations of the gases to one another were afterwards tried. Faraday observes that—

"Notwithstanding the many disturbing causes which interfere with first and hasty experiments of this kind, and produce results which occasionally cross and contradict each other, still there are some very striking considerations which arise in comparing the gases with each other at the same temperature. Foremost amongst these is the place of oxygen; for of all the gaseous bodies yet tried it is the least diamagnetic, and seems in this respect to stand far apart from the rest of them. The condition of nitrogen, as being highly diamagnetic, is also important. The place of hydrogen, as being less diamagnetic than nitrogen, and of chlorine, which, instead of approaching to oxygen, is above hydrogen, and also of iodine, which is probably far above chlorine, are marked circumstances.

"*Air* of course owes its place to the proportion and the individual diamagnetic character of the oxygen and nitrogen in it. The great difference existing between these two bodies in respect of magnetic relation, and the striking effect presented by oxygen in coal-gas and hydrogen, bodies not far removed from nitrogen in diamagnetic force, made me think it might not be impossible to separate air into its two

chief constituents by magnetic force alone. I made an experiment for this purpose, but did not succeed; but I am not convinced that it cannot be done. For since we can actually distinguish certain gases, and especially these, by their magnetic properties, it does not seem impossible that sufficient power might cause their separation from a state of mixture."

He next tried the effect with the gases heated, and found it a general rule, as far as his experiments went, that hot gas was diamagnetic to cold.

In conclusion he observes: "Such are the results I have obtained in verifying and extending the discovery made by P. Bancalari. I would have pursued them much further, but my present state of health will not permit it: I therefore send them to you with, probably, many imperfections. It is now almost proved that many gaseous bodies are diamagnetic in their relations, and probably all will be found to be so. I say almost proved; for it is not, as yet, proved in fact. That many, and most, gaseous bodies are subject to magnetic force, is proved; but the zero is not yet distinguished. Now, until it is distinguished, we cannot tell which gaseous bodies will rank as diamagnetic and which as magnetic; and, also, whether there may not be some standing at zero. There is evidently no natural impossibility to some gases or vapors being magnetic, or that some should be neither magnetic nor diamagnetic. It is the province of experiment to decide such points; and the affirmative or negative may not be asserted before such proof is given, though it may, very philosophically, be believed.

"For myself I have always believed that the zero was represented by a vacuum, and that no body really stood with it. But though I have only guarded myself from asserting more than I knew, Zantedeschi, (and I think also De la Rive,) with some others, seem to think that I have asserted the gases are *not* subject to magnetic action; whereas I only wished to say that I could not find that they were, and perhaps were not: I will therefore quote a few of my words from the Experimental Researches. Speaking of the preparation of a liquid medium at zero, I say, 'Thus a *fluid* medium was obtained, which practically, as far as I could perceive, had every magnetic character and effect of a gas, *and even of a vacuum*,' &c.—Experimental Researches, 2423. Again, at 2433, I say, 'At one time I looked to air and gases as the bodies which allowing attenuation of their substance without addition, would permit of the observation of corresponding variations in their magnetic properties, but now all such power by rarefaction *appears* to be taken away.' And further down at 2435, 'Whether the negative results obtained by the use of gases and vapors, depend upon the *smaller quantity of matter* in a given volume, or whether they are the direct consequences of the altered physical condition of the substance, is a point of very great importance to the theory of magnetism. I have imagined in elucidation of the subject an experiment, &c., but expect to find great difficulty in carrying it into execution,' &c. Happily P. Bancalari's discovery has now settled this matter for us in a most satisfactory manner. But where the true zero is, or that every body is more or less removed from it on one side or the other, is not, as yet, experimentally shown or proved.

"I cannot conclude this letter without expressing a hope that since gases are shown to be magnetically affected, they will also shortly be found, when under magnetic influence, to have the power of affecting light, (Experimental Researches, 2186, 2212.) Neither can I refrain from signalizing the very remarkable and direct relation between the forces of heat and magnetism, which is presented in the experiments on flame, and heated air and gases. I did not find on a former occasion (Experimental Researches, 2397) that solid diamagnetic bodies were sensibly affected by heat, but shall repeat the experiments and make more extensive ones, if the Italian philosophers have not already done so. In reference to the effect upon the diamagnetic gases, it may be observed that, speaking generally, it is in the same direction as that of heat upon iron, nickel and cobalt; *i. e.*, heat tends in the two sets of cases, either to the diminution of magnetic force, or the increase of diamagnetic force; but the results are too few to allow of any general conclusion as yet.

"As air at different temperatures has different diamagnetic relations, and as the atmosphere is at different temperatures in the upper and lower strata, such conditions may have some general influence and effect upon its final motion and action, subject as it is continually to the magnetic influence of the earth."

2. *Effect of the Fusion of Siliceous Minerals on their specific gravity*, (Berz. Jahresbericht, 1846, p. 315.)—The fusion of a crystallized mineral produces, as has been known, a glass of different specific gravity. According to the trials of Deville, (Jour. f. Pract. Chem., xxxvi, 295,) there is a change in

	Sp. gr. of crystals.		Sp. gr. after fusion.		
Labradorite,	from 2·6894	to	2·5255	or about	$\frac{1}{16}$ less.
Feldspar,	" 2·561	"	2·351	"	$\frac{1}{12}$ "
Hornblende,	" 3·2159	"	2·8256	"	$\frac{1}{8}$ "
Pyroxene,	" 3·2667	"	2·8055	"	$\frac{1}{7}$ "
Peridot from Fayo,	" 3·3813	"	2·8517	"	$\frac{2}{13}$ "

Chrysolite is commonly infusible, and this Fayo specimen must have consisted largely of silicate of iron.

Brongniart has shown the corresponding fact that porcelain biscuit has a specific gravity of 2·62; but after baking, it is only 2·242.

According to Rose, (Pogg. Ann., lxvi, 97,) the Berlin porcelain changes in the same manner. Feldspar diminishes according to his experiments $\frac{1}{13}$ by fusion; porcelain earth, which does not fuse, has its specific gravity diminished from 2·633 to 2·564. The mixture for making porcelain, after baking, is reduced to a specific gravity 2·452, which exceeds the diminution in the ingredients alone, and indicates according to Rose, the union of the alumina into a new chemical compound, which in a semi-vitreous condition has this small specific gravity.

3. *Metacetonic and Butyro-acetic acids.*—The identity of these acids has been ascertained by MM. Dumas, Malaguti and Leblanc.

4. *On the Estimation of the amount of Gypsum in Culinary Salt;* by M. Lassaigne, (Chem. Gaz., Nov. 1, 1847, p. 433, from Jour. de Chim. Méd., viii, p. 425, August, 1847.)—To determine the amount of gypsum occurring naturally or as an adulteration in common salt,

the salt is dissolved in a solution of sulphate of lime saturated at 68° F., and the residue of gypsum washed with the same liquid. In this manner the loss which would occur on edulcoration with water is avoided. Nine grms. of salt were mixed with one grm. of sulphate of lime, both rubbed to powder, and separated with the above liquid; 0·995 grm. gypsum was re-obtained. The method may therefore be employed in practical investigations of this kind with advantage and satisfactory accuracy.

5. *Method of detecting the Adulteration of Cane-sugar with Starch-sugar, and the Syrup of the former with that of the latter*; by Dr. G. Reich, (Chem. Gaz., Nov. 1, 1847, p. 433, from Archiv der Pharm., l, p. 293.)—When a boiling saturated solution of bichromate of potash is added to a thick syrup of cane-sugar and the mixture heated to boiling, the chromic acid is reduced with a violent reaction, which continues even after removing the vessel from the fire, and the liquid is colored green by the oxyd of chromium in solution. When starch-sugar syrup is treated in the same manner, a very different behavior is observed, and which may be employed to distinguish these two kinds of sugar; the starch-molasses has not the least action on the bichromate of potash; and when mixed with cane-sugar molasses to the amount of $\frac{1}{3}-\frac{1}{5}$, prevents the reaction of the latter; the mixture merely froths somewhat more when heated, but does not change its color. When the amount of starch-sugar molasses is smaller, a slight alteration of color is perceptible; but it is never so decided as with cane-sugar syrup alone. While these syrups may be thus distinguished, the sugars themselves cannot be determined in this manner, because the bichromate of potash is wholly without action upon the concentrated solutions of both, but they may be distinguished by nitrate of cobalt. When some potash is added to a concentrated solution of pure cane-sugar, and the mixture heated to boiling, a violet-blue precipitate falls, even when the solution is diluted, on the addition of a few drops of nitrate of cobalt. The dilute solution of the starch-sugar, treated in the same manner, is not precipitated; and the concentrated solution of the same sugar, to which potash has been added, deposits a dirty brown precipitate. A small amount of starch-sugar in the cane-sugar prevents the formation of the violet-blue precipitate with nitrate of cobalt. The above experiments of Reich have been repeated and confirmed by Herzog, who observes that the syrup of beet-root sugar acts as violently on the bichromate of potash as the syrup of cane-sugar; the liquid, however, does not become green, but retains its color like the syrup of starch-sugar. A mixture of cane-sugar syrup and the syrup of beet-root sugar must however contain far more of the latter for the green coloring produced by the cane-sugar not to appear. Herzog also examined the method proposed by Reich to distinguish grape-sugar and cane-sugar with nitrate of cobalt, and likewise recommends it. Beet-root sugar and mannite afford, according to Herzog, when mixed with potash, the same precipitate with solution of cobalt as cane-sugar. Milk-sugar prevents the precipitation of the nitrate of cobalt by potash, like grape-sugar. Herzog at the same time asserts that mannite, when boiled with sulphate of copper and potash, immediately reduces the oxyd of copper, which is not the case accord-

ing to Dr. Knop when the mannite is pure and perfectly free from grape-sugar.

6. *Patent granted to James Napier, Middlesex, for Improvements in smelting Copper and other Ores*, (Chem. Gaz., Nov. 1, 1847, p. 435.) —This invention consists in improvements in smelting copper ores, by treating them with fluxes consisting of common salt, lime and carbonaceous matters; and also in improvements in smelting ores containing silver or gold, or both those metals, by the addition of alkaline substances, coal, iron and galena.

The first object is to facilitate the separation of the earth from the copper; and to effect this, when several ores of different descriptions are to be operated upon, the patentee mixes them in such proportions, in relation to the earthy matters or gangue they contain, as will cause the earths to unite in the furnace and form glass; the ores have been mixed in suitable proportions, when the silica in the mixture ranges from 50 to 75 per cent., in relation to the other earthy matters, which are generally mixtures of alumina, lime, baryta, fluor-spar, &c.; the presence of oxyd of iron greatly facilitates the fusion of the ores. Should the mixture (or the ore, when only one description of ore is being treated) not contain silica in the above proportion, the deficiency is to be supplied by the addition of sand; or if the silica exceeds the above proportion, lime or fluor-spar is to be added.

After the above preparatory process, the operation is conducted in the following manner:—If the ore or ores should contain not less than 1 part of iron and 1 part of sulphur to 2 parts of copper, an addition is made to every ton of the ore of 56 lbs. common salt, 40 lbs. of slaked lime, and 100 lbs. of coal, and the whole is fused in a melting furnace. When fused, the slag or scoria is skimmed off, and the furnace is tapped into sand molds. The ingots or pigs thus produced are treated as hereafter described. If the ore or ores should contain less than 1 part of iron to 2 parts of copper, the deficiency is to be supplied by the addition of sulphuret of iron; or the ore is to be treated as before mentioned (omitting the coal); and after the fused mass has been skimmed, 30 lbs. of scrap iron are to be dispersed over the surface thereof as equally as possible, and the door of the furnace is closed until the scrap iron is melted; the furnace is then to be tapped into sand molds. When the ingots obtained in the above manner are set, they are thrown into water, whereby they become disintegrated and fall into a fine powder; this powder is thrown into a heap, and allowed to remain for forty-eight hours; after which it is removed to a calcining furnace, and treated in the manner described in the specification of a patent obtained by the present patentee, July 20, 1846. The addition of black oxyd of manganese, instead of iron, has been found to produce a similar effect, but not with equal advantage.

When ores containing little or no sulphur are operated upon, the above-mentioned processes of disintegration and calcination are omitted. The patentee commences, in this case, by mixing the ores in relation to their earthy matters so as to form glass as above described (the ores, when containing no iron, might with advantage have a small quantity of oxyd or carbonate of iron added); and then 80 lbs. common salt, 50 lbs. slaked lime, and 100 lbs. anthracite coal, finely pul-

verized, are added to each ton of ore containing 10 per cent. of copper. If the ore should be richer in copper, a smaller proportion of salt and lime will suffice, and a greater proportion of anthracite coal will be required. The patentee says he has found that for an ore containing 25 per cent. of copper, 56 lbs. common salt, 50 lbs. slaked lime and 150 lbs. anthracite coal will answer well. The mixture of ore and other materials is fused in a melting furnace, which for a charge of 25 cwt. of ore will take from five to six hours; and then the fused mass is tapped into sand molds. The copper thus obtained will generally be ready for the refining operation; but should a portion of the produce be regulus, it is to be roasted, and afterwards refined. Soda and several of its salts may be used instead of common salt; and so likewise may potash and several of its salts, or mixtures of these, free from sulphur.

Sulphuretted ores of copper, containing silver or gold, or both these metals, are treated in the following manner:—The ore is first calcined and fused, as in the ordinary smelting process, so as to produce a regulus containing about 50 per cent. of copper; with every ton of this regulus, 56 lbs. soda-ash, 40 lbs. slaked lime, 1 cwt. coal, mixed 1½ cwt. iron in scraps and 4 cwts. galena (sulphuret of lead) are mixed, and the mixture is fused in a fusing furnace until the iron disappears; the fused mass is then well rabbled, and tapped into sand molds. The lead will be found reduced at the bottom of the first and second ingots, and will contain all, or the greater part, of the silver or gold, or both, which the ore previously contained; these metals are afterwards separated from the lead by the ordinary methods of separating silver and gold from lead. The copper is treated in the ordinary manner, or as described in the specification of the patent before alluded to. Instead of galena the oxyd of lead may be employed, in which case the iron is dispensed with; but the patentee prefers to use galena.

When treating ores of silver or gold, or both, which do not contain copper, or which do not contain it in the state of a sulphuret, the patentee adds copper pyrites thereto, in the proportion of 4 cwts. of the latter to 16 cwts. of ore, and then proceeds in the manner above described, viz., bringing the material into a state of regulus, and fusing it with soda-ash, lime, coal, iron and galena.

7. *Action of Sulphuretted Hydrogen upon Nitric Acid, with a new process for obtaining Sulphur from Sulphate of Lime;* by C. Lecomte, (Ann. de Chim. et de Phys., Oct., 1847.)—M. Millon has stated that sulphuretted hydrogen is without action upon nitric acid deprived of hyponitric acid. This is disproved by our author, who experimented upon nitric acid, sp. gr. 1·33, freed from hyponitric acid by nitrate of urea. The acid was kept at the temperature of 64°–68° Far.

The first bubbles of the gas gave rise to action; sulphur separated, and the acid became intensely green, with the evolution of orange colored vapor.

The sulphur separated has, although prepared at a low temperature, the elasticity of that which has been poured while hot into cold water.

The gases at first given off, were deutoxyd of nitrogen and hyponitric acid, the last giving way to sulphuretted hydrogen as the action diminished.

The following experiment proved that these last named gases are incompatible. A jar filled with deutoxyd of nitrogen and sulphuretted hydrogen, shewed no action; a few bubbles of oxygen colored the gases, caused sulphur to deposit, and the color disappeared. Each addition of oxygen produced the same effects, until all the sulphuretted hydrogen was removed.

Upon this reaction the author bases his process for obtaining sulphur from sulphate of lime. The sulphate is to be decomposed with coal, and the moist sulphuret by the carbonic acid from a furnace. The sulphuretted hydrogen given off, is to be conducted into a chamber containing air and deutoxyd of nitrogen; which last will, in small quantity, suffice as in the manufacture of sulphuric acid, for the preparation of large quantities of product. In this case water is formed, and sulphur deposited.

The nitric acid which had been acted upon by sulphuretted hydrogen in the above experiment, contained sulphate of ammonia, with scarcely a trace of nitrate. G. C. SCHAEFFER.

8. *New and Economical Preparation of the Chromates;* by V. A. JACQUELAIN, (Comptes Rendus, Oct., 1847.)—An intimate mixture of finely pulverized chalk and chrome ore, is to be exposed in thin layers in a reverberatory furnace, to a red heat for nine or ten hours, stirring ten or twelve times with a rake.

The tests of the perfect conversion of the oxyd into chromate are, the yellowish green color of the mass and its perfect solubility in hydrochloric acid.

The mass when ground in a mill, is mixed with hot water, constantly agitated, and sulphuric acid added to slight acid reaction. The chromate of lime is thus changed to bichromate; some pulverized chalk is then gradually added, to remove peroxyd of iron, and the liquid after standing to clear is decanted.

The chromates of lead and zinc may now be prepared at once, the intermediate transformation into bichromate of potash being economically avoided. This latter salt may however be readily prepared from the liquid, and quite pure, by solution of carbonate of potash, which produces insoluble carbonate of lime and solution of bichromate of potash.

The chromate of lime, with excess of base, has the green color of oxyd of chrome, and as it is nearly insoluble in water, has probably led to the belief that no chromate was formed. G. C. S.

9. *Preparation of Chloric Acid and the Chlorates;* by LEWIS THOMPSON, (Phil. Mag., December, 1847, Supple.)—Dissolve in the least possible quantity of boiling water equivalents (122·81) of chlorate of potash and (168·34) of bitartrate of ammonia. Mix the solution, and let the bitartrate of potash crystallize out. Mix the clear solution with an equal bulk of alcohol, and filter. Boil the alcoholic solution of chlorate of ammonia in a narrow necked vessel with excess of fresh carbonate of baryta until the ammonia is expelled; filter, evaporate, and crystallize.

The chlorates of strontian and lime are prepared in the same way, the metallic chlorates by double decomposition from chlorate of baryta.

Chloric acid is to be prepared by adding to chlorate of baryta just enough sulphuric acid to combine in the baryta—a long time is required to complete the decomposition—after this filter and evaporate.

If the acid is added as long as a precipitate falls, an excess will be found in the liquid.

For pyrotechnical use the chlorates may be prepared without the use of alcohol. G. C. S.

10. *On the Hydrates of Nitric Acid;* by ARTHUR SMITH, (Phil. Mag., December, 1847.)—With a view to determine the composition of these hydrates especially the first hydrate about which some doubt has existed, the author commenced his experiments upon a quantity of pure red fuming acid. After several distillations, the acids obtained each time being decolored by a current of dry air drawn through the warmed liquid, the strongest pure colored acid obtained was found to have almost exactly the composition $NO_6 H$. Its boiling point was 184°, sp. gr. 1·517 at 60°. It had not the slightest action upon tin or iron even when boiled. No change took place in it on exposure to a freezing mixture of ice and salt.

An attempt was made to form a deutohydrate by adding water in proper proportions to acid of known strength, but on distillation the boiling points rose rapidly. We maintain there is not a true compound but divides itself spontaneously into the first hydrate and the one next mentioned. A freezing mixture produced no effect.

The tetrohydrate $NO_6 H+3HO$ was formed by adding the proper quantity of water to a strong acid. Its density was 1·424 at 60°—the boiling point uniformly, 250°. A very weak acid boiled for several hours was brought nearly to this hydrate. Stronger acids are also brought to the same strength by boiling, as they lose acid. This therefore may be considered as the proper hydrate of nitric acid.

G. C. S.

11. *On the action of Anhydrous Phosphoric Acid upon the Amides and Ammoniacal Salts;* by MM. DUMAS, MALAGUTI and LEBLANC, (Chem. Gaz., Nov. 1, 15, and Dec. 1, from Compt. Rendus de l'Acad., Sept. 13, 27, and Oct. 4, 1847.)—By acting with anhydrous phosphoric acid upon crystallized acetate of ammonia, M. Dumas obtained a volatile liquid which boiled at 171° and was miscible with water in all proportions. When purified by rectification over chlorid of calcium, it gave on analysis the following numbers:

Carbon, . .	57·4 . .	4 = 58·5
Hydrogen, . .	7·4 . .	3 = 7·3
Nitrogen, . .	34·4 . .	1 = 34·2

The density of its vapor was found to be 1·45.

These numbers lead to the very simple formula $C_4 H_3 N$, which represents four volumes of vapor, and is derived from that of acetate of ammonia by the abstraction of the elements of four equivalents of water. M. Dumas however regards its *rational formula* as $C_2 NH$, CH_2, that is to say, the hydrocyanate of methylene. This compound, corresponding to the hydrocyanic ether of M. Pelouze, was prepared by distilling dry cyanid of potassium with sulphate of methylene (sulphomethol), and when carefully purified was found to be perfectly identical in all its characters with the substance from acetate of ammonia.

A boiling solution of caustic potash decomposes it with the formation of an alkaline acetate and the evolution of ammonia; chromic acid and boiling nitric acid have no action upon it. It is violently decomposed by potassium in the cold; cyanid of potassium being formed while free hydrogen and a carburet of hydrogen are evolved.

Chloracetate of ammonia by the same process gives an analogous body which is $C_4\,Cl_3\,N$. It is liquid, boils at 178°, and has a specific gravity of 1·441; by the action of a solution of potash, it regenerates chloracetic acid with the evolution of ammonia.

The authors have examined the reaction of butyrate and valerianate of ammonia with anhydrous phosphoric acid, and have obtained in this way bodies in every respect analogous to the last, having respectively the formulas $C_8\,H_7\,N$ and $C_{10}\,H_9\,N$. The latter is identical with the valeronitrile obtained by Schliesser from the action of chromic acid upon gelatine.* By the action of a solution of potash they evolve ammonia and regenerate the acid of the original salt; by potassium they are decomposed with the formation of a cyanid, and the evolution of free hydrogen and a carburet of hydrogen which is more dense as the equivalent of the acid is higher.

They have also submitted the amides of these acids to the action of the anhydrous phosphoric acid; as these differ from the new compounds only by the elements of two equivalents of water, it was supposed that they would readily yield them by this process, and accordingly they find that butyramide and chloracetamide afford the same products as the corresponding ammoniacal salts. Benzamide and benzoate of ammonia, by the same reaction produce the benzonitrite of Fehling, which is quite analogous to the compounds above described.

[The results of these researches are of great interest not only as furnishing us the means of dehydrating the ammoniacal salts generally, but as enabling us to complete a series of which but two or three members were hitherto known, the anhydrid amids of that group of monobasic acids whose formula is represented by $(CH)nO_4$. In this Journal for January, p. 119, we noticed that the researches of Frankland and Kolbe prove the cyanid of ethyle of Liebig (hydrocyanate of etherene of Dumas), to be really the anhydrid amide of metacetonic acid, homologous with valeronitrile and probably with hydrocyanic acid. These bodies are represented by the general formula $C_n\,H_{n-1}\,N$; they may be conveniently designated in conformity with the name of M. Schliesser, as acetonitrile, metacetonitrile.

It is seen from the above researches, that these bodies are obtained by combining the cyanic elements with alcohol; the elements of an equivalent of alcohol with one of hydrocyanic acid, minus two of water, equal the *nitrile* of the acid belonging to the group next higher in the series. Thus the methylic compound is the anhydrid amide of the acetic acid, and the ethylic cyanid corresponds to the acid of the metacetonic series. The anhydrid of the caprate of ammonia will be found in the cyanid of amylic alcohol, and if we look in this way for the *alcohol* corresponding to the formic anhydrid, we shall find it to be *water*, represented as $H_2\,O_2$, and this must be regarded as its equiva-

* See this Journal for January, 1848, p. 118.

lent while we preserve the present formulas for organic compounds. The group of alcohols are referable to the common formula $C_n H_{n+2} O_2$, (n being always two or a multiple of it) or $((CH)_2)\,n\,H_2\,O_2$. Wine alcohol being represented by $C_4 H_6 O_2$, if we subtract from it $(CH)_2$ there remains the formula of wood-spirit $C_2 H_4 O_2$, and again subtracting $(CH)_2$, there is left $H_2 O_2$ or water.

In conclusion we would again call attention to the fact mentioned in the remarks at page 119 of the last number; M. Liebig states that the cyanid of ethyle (metacetonitrile) is decomposed by oxyd of mercury; if as is probable, it combines with the cyanic elements and regenerates the alcohol, we are now put in possession of a process by which we can obtain all the higher alcohols of the series, most of which are at present unknown. Farther, the apparent homology between hydrocyanic acid and this new series of bodies, suggests a new and important field for inquiry which we trust will not long be neglected.]

T. S. Hunt.

12. *On Xyloidine, Pyroxyline, and some analogous products;* by F. Dumonte and Mendrel, (Compt. Rend. des trav., &c., Aug., 1847, from Recueil des trav. de la Société d'Emul. pour les Scien. Pharm., Avril, 1847, p. 104.)—According to the authors, gun-cotton is partly dissolved by the action of alcoholic ether. The insoluble part yielded on analysis, carbon 28·5, hydrogen 3·5, nitrogen 11·6, corresponding to the formula, $C_{24} H_{16} O_{16} + 2NO_5$, or representing NO_2 by X, and dividing the equivalents after the manner of M. Gerhardt, $C_{12}(H_{16} X_4) O_{10}$. It is therefore *quadrinitric lignine.* This substance is white, burns readily, and dissolves with ease in fuming nitric acid; it is identical with the xyloidine of M. Braconnot.

The soluble part precipitated by water, gave numbers corresponding to the formula, $C_{24} H_{18} O_{18} + 6NO_5$, or as above, $C_{12}(H_{18} X_6) O_{12}$, which represents *sexnitric glucose.*

The same chemists have also obtained *quintinitric* mannite, $C_{24}(H_{18} O_{14} + 10NO_5$, or $C_6(H_9 X_5) O_6$. To prepare this, carefully purified and dried mannite is gradually added to fuming nitric acid, taking care to cool the vessel so as to prevent the evolution of red fumes. When the mannite is dissolved, the solution is precipitated by adding its volume of strong sulphuric acid, which separates the quintinitric mannite in white flakes. It is to be thoroughly washed in water, and finally dissolved in boiling dilute alcohol, which on cooling deposits it in very minute silky needles. It is insoluble in water, but in boiling water fuses like wax and falls to the bottom of the vessel. It explodes readily by percussion, evolving sufficient heat to inflame gunpowder, and M. Sobrero proposes it as a substitute for fulminating mercury in percussion caps. In a paper before the French Academy, (Compt. Rendus, Juillet 19, 1847,) he states, that nitric mannite can be advantageously substituted for mercurial fulminate, and arrives at the following conclusions: first, it must always be cheaper than fulminating mercury; secondly, it is more conveniently prepared, and does not expose the workmen to the great danger which attends the preparation of the mercurial salt.

It requires for detonation a violent blow between two hard bodies; it may be placed on paper and touched with a red-hot coal, and fused

without explosion; the paper may even be burned; it is decomposed without detonation. Lastly, it does not evolve any nitrous vapors when exploded, but appears to be entirely resolved into carbonic acid, water and azote, besides which, it does not change by keeping.

The increase of weight during the transformation of mannite into the new substance, is very considerable; 100 parts become 225.

T. S. H.

13. *On the Conversion of Neutral Nitrogenous Bodies, as Fibrine and Caseine, into Fatty Substances;* by M. BLONDEAU, (Chem. Gaz., Nov. 1st, 1847; from Compt. Rend., Sept. 6th, and Journ. de Pharm., Nov.)—The attention of M. Blondeau was first called to these facts while examining the changes which take place in the manufacture of Roqueforte cheese. This is prepared from ewe's milk coagulated by rennet, and the fresh curd contains but $\frac{1}{200}$th of fatty matter, as M. Blondeau assured himself by treating it with a mixture of boiling alcohol and ether. The peculiar qualities which render it so much prized, are acquired by placing it for some weeks in cool, damp, and dark cellars; when the cheese has been only five days in this situation, it is found to contain a considerable portion of a fat body mixed with unaltered caseum, and at the end of two months it is almost completely converted into a fatty substance, which was readily separated from the unchanged caseum by boiling with water. It has a sweet agreeable taste like butter, melts at 104° F., begins to boil at 176°, and is decomposed at about 302°; it is easily saponified, and presents a considerable resemblance to butter, but differs in many respects.

This change the author considers dependent upon the development of several mycodermic plants which grow with such rapidity, that it is found necessary to scrape the cheese from time to time, but the plants reappear and grow with fresh energy. They consist of the *Peucillium glaucum*, and another species which he has named *globulosum*, with two species of *Torrula*, one with green sporules denominated *viridis*, and another with orange colored sporules which he has called *aurantiaca*. All of these vegetables contain nitrogen; their development consequently requires the presence of ammonia and carbonic acid. The ammonia can only be furnished by the caseine which, in parting with this alkaloid, becomes converted into a fatty body; its composition in fact approaches very nearly that of the fats, if we subtract the elements of ammonia. The carbonic acid may be derived either from the caseine or from the air.

In order to determine whether fibrine could undergo a similar change, a pound of beef free from fat was slightly salted, was covered with a coating of paste and placed in a cellar. After two months the meat had undergone no putrid decomposition, but was surrounded by a green mould of *Torvula viridis*, and for the greater part converted into a fatty body greatly resembling hog's lard. This experiment appears to decide the long disputed question, whether the bodies of animals under similar circumstances may be entirely changed into a fatty substance.

T. S. H.

14. *The Decomposing power of Water at high Temperatures*, (Trans. Am. Phil. Soc., vol. x, Part II; by RICHARD TILGHMAN, A.M.)—This paper shows that the vapor of water passed over various substances at

a high heat, has power to decompose them with the evolution of their constituents. The anhydrous chlorid of calcium, and the chlorids of strontium and barium were rapidly decomposed by passing over them at a high red heat, a current of steam: hydrochloric acid was copiously evolved, while the bases were left pure, and the lime anhydrous. The same decomposition was also observed in case of various sulphates, (oxysalts as they are usually called,) e. g., sulphates of magnesia, lime, strontia, and baryta. The heat required for their decomposition, is higher as the amount of their basic power is greater. The sulphate of magnesia gives off its acid in a current of steam at a low red heat, while the sulphate of lime requires a high red heat, and of baryta a low white heat. At a full white heat the subphosphate of lime was decomposed, with slow evolution of its phosphoric acid. Lime, magnesia, and particularly alumina, were found to aid materially by their presence, in the decomposition of the sulphates and chlorids of potash and soda, which alone suffer only an imperfect decomposition by the agency of hot watery vapor, because of the volatility of the hydrates of these alkalies at a high heat.

As potash alum is completely decomposed by steam at a high temperature, analogy of constitution led our author to the belief that feldspar (which has the same constitution with potash alum, changing silicic acid for sulphuric) would also suffer the same change, and experiment confirmed the conjecture. Partial fusion of the fragments of feldspar was the only visible change now produced; these fragments however being pulverized and boiled in water, the concentrated solution was strongly alkaline, and proved to contain aluminate of potash. Dilute sulphuric acid also produced a small portion of alum from the powder which had been exhausted with water.

[The value of Mr. Tilghman's observations, will be obvious to all who feel any interest in the chemico-geological changes which the rocks of the globe have undergone. In connection with this subject, we would allude to the experiments of Dr. Turner, on the solvent power of high steam, and also to the papers on Pseudomorphism and the metamorphic agency of hot waters.* Especially would we allude to the experiments of Mr. J. Jeffreys, detailed in the Brit. Asso. Report for 1846, 2d part, p. 125, an abstract of which will be found in this Journal, i ser., xlviii, 397.—B. S., Jr.]

II. Mineralogy and Geology.

1. *Brandisite, a New Mineral;* by M. Haidinger, (Neues Jahrbuch, für Min. Geog., &c., 1847, p. 349.)—Brandisite occurs at Monzoni in the Fassa-thal. Its crystals are six-sided prisms, secondary to a rhombohedron, with a transverse cleavage. Lustre, pearly and vitreous. Color, leek green to reddish gray, or leek green in the direction of the axes, and brown transversely. H. = 4·5. G. = 3·015 – 3·062. Thin folia not elastic. Affords the reaction of silica, alumina and iron before the blowpipe.

* See this Journal, i ser., xlv, 104; xlviii, 82.

2. *Manganocalcite* of BREITHAUPT, (L'Institut, No. 726, from Poggend. Ann., lviii, 511.)—This mineral from Schemnitz, contains carbonic acid and protoxyd of manganese 67·48, carbonic acid and lime 18·81, magnesia 9·97, carbonic acid and protoxyd of iron 3·22.

3. *Stiblite, a New Mineral;* by MM. BLUM and DELFSS, (L'Institut, No. 726.)—Stiblite is an ore of antimony from Zamara in Spain, Felsobanya and Kremnitz in Hungary, in Bavaria, and Mexico. Analysis shows that it is an antimonate of antimony, affording the formula $SbO_3 + SbO_5 + 2HO$. The name alludes to its containing antimony (stibium). It was first mistaken for oxyd of antimony.

4. *Hydrosilicate of Alumina, allied to Halloysite*, (Bull. Soc. Geol. de France, ii ser., iv, p. 464.)—A mineral of this kind from Montmorillon (Vienna) is amorphous and fragile; its color is rose red. It falls to pieces readily in water, but does not become plastic.

M. Damour obtained for its composition, as the mean of three analyses, silica 50·04, alumina 20·16, peroxyd of iron 0·68, lime 1·46, magnesia 0·23, potash 1·27, water 26·, =99·84, leading to the general formula $\dot{r}\ddot{S}^3 + 4\mathbf{R}\ddot{S}i^2 + 28\mathbf{H}$.

A halloysite from the pumaceous tufa of Milo, afforded M. Lauvage, silica 31·6, alumina 23·2, magnesia and alkali 3·20, quartz 29·30, water 12·70, affording the formula $Si^3\ Al^2\ Aq^2$.

5. *Sulphuret of Cobalt;* (Chem. Gaz., No. 77, p. 23.)—A protosulphuret of cobalt has been found near Rajpootanah in Hindoostan, in amorphous masses. The color is steel gray. It consists of cobalt 64·75, sulphur 35·26.

6. *Dufrenoysite;* by M. DAMOUR, (Ann. de Ch. et de Phys., xiv, 379.)—Dufrenoysite is an arsenical sulphuret of lead, consisting of lead 55·40, sulphur 22·49, arsenic 20·69, silver 0·21, copper 0·31, iron 0·44, which differs from feather ore only in containing arsenic in place of antimony. It has a gray color like copper glance, metallic lustre, reddish brown powder, and the specific gravity 5·549. Its crystals are rhombic dodecahedrons. It melts easily before the blowpipe and affords fumes of arsenic and sulphur. It is from the dolomite of St. Gothard, associated with realgar, zinc, blende, and pyrites.

7. *Smælite;* by M. GLOCKER, (Jour. f. Prakt. Chem., xxxv, 39.)—Smælite is an amorphous grayish white mineral, allied to halloysite. It is a hydrous silicate of alumina, containing silica 50·0, alumina 32·0, soda 2·0, peroxyd of iron 2·0, water 13·9 = 99·9. H. = $2\frac{1}{2}$. G. = 2·168. The name is from the Greek σμηλη, *soap*. It occurs in a trachytic porphyry near Tekibanga in Hungary.

8. *Coal in the East Indies;* (L'Institut, No. 726.)—Coal has been found by a Siamese of Penang, on the south side of the island of Junk Ceylon, well known for its tin. The bed is three feet thick. The coal is of a black or brownish black color, burns with a clear flame, and seems to be very bituminous.

9. *Researches among the Protozoic and Carboniferous Rocks of Central Kentucky, made during the summer of* 1846; by D. D. OWEN, M.D., and J. G. NORWOOD, M.D. 12 pp. 8vo, with a plate. St. Louis. 1847.—The authors have detected the "cliff" formation of the west (upper Silurian) in Davidson and Sumner counties, Tennessee. But

the "knobby region" of Indiana, Kentucky, Tennessee, Illinois, and Ohio, above the black slate, they show to correspond to the carboniferous rocks. Consequently, they remark, "that most, if not all, of the groups of rocks which occur in New York, from the Genesee slate to the top of the Catskill range, are deficient, or obscurely marked in the west; and the carboniferous rocks rest almost immediately on the schistose beds, which represent the Genesee slate; whilst our black slate and the underlying shell-beds of the Falls of the Ohio, with the Goniatite limestone of Rockford, Jackson Co., Indiana, as well as the upper glades of Perry Co., Tenn., containing the *Calceola sandalina*, *Atrypa Wilsoni*, *Phacops macrophthalma*, *Pleurorhynchus*, *Pentremites Rheinwardi*, *Atrypa prisca*, *Spirifer* (like *Ostiolata* of Schlotheim), *Pileopsis prisca*, &c., are the representatives of the Devonian system of Europe.

In a section of the country from Nashville in Tennessee, to Salt river in Kentucky, a distance of one hundred and fifty miles, they represent a lower Silurian blue limestone formation as extending from Nashville beyond Gallatin; soon after, near the Kentucky line, the cliff formation (upper Silurian) appears with the overlying black slate (Devonian); at the Kentucky line commences the lower division of the carboniferous rocks—"the formation of the knobs;" it has an anticlinal axis at Barren river, and beyond Glasgow, Ky., is covered by another carboniferous deposit, mostly of limestone, which is "the formation of the Barrens;" it is inferior in relative position to the coal beds. This formation of the Barrens extends on forty-five miles, to the summit of Muldrow's Hill, Ky., where the lower division of the carboniferous rocks outcrops; soon after appears the black slate, and next the cliff formation (upper Silurian), and beyond Bardstown, about twenty miles from Muldrow's Hill, is the blue limestone formation, (lower Silurian.)

10. *Temperature of the Geysirs of Iceland;* by M. Descloizeaux, (Phil. Mag., xxx, p. 397, from Ann. de Chem. et de Phys., for April, 1847.)—In each of the five experiments which we made at the Great Geysir, we employed five thermometers arranged on a single line, and separated by nearly equal intervals.

It is known that the Great Geysir has formed, by successive incrustations, a very regular basin, having the shape of a truncated cone, the exterior slopes of which have a mean inclination of 8°, whose summit is occupied by a cup or vase,* presenting only on the edges some small clefts through which is carried the overflow of the water, when its level rises. The mean inclination of the sides of this cup is 13°.

In the centre of the basin is a circular well, which appears perfectly regular as far as the eye can discern. Between two consecutive eruptions, this well, and the cup which surrounds it, are completely filled with water, a very small portion only of which overflows after the upheavings, which take place about every hour and a half; but immediately after the great eruptions, the cup is completely empty, and in the well the water descends below the bottom of the cup from one metre to 2 m·50, according as the projected column had attained a greater or less height; the former level is not restored until after six or seven hours.

* Sir W. J. Hooker thus describes it:—"To compare great things with small, the shape of this basin resembles that of a saucer with a circular hole in its middle." Tour in Iceland, p. 117.—Tr.

The following are the principal dimensions which we have found for different parts of the Geysir :—

	m
Diameter of the basin or cup, on the edges, from north to south,	16·00
Diameter of the basin or cup, in a perpendicular direction,	18·00
Diameter of the central well,	3·00
Depth of the well, including that of the basin :—	
1. In the direction of the first diameter,	23·50
2. In the direction of the second diameter,	21·50
Depth of the cup in the centre,	1·57

As we took care, in each experiment, to alter the order of our thermometers, that their indications might be checked, I shall indicate by numbers and by the letter B, those of M. Bunsen, and by the letter D, those which belonged to me.

It will be conceived that the temperature at the surface of the water which fills the basin, must be excessively variable ; for that temperature depends at the same time on the temperature of the sides of the basin, on that of the air, on the force and direction of the wind, and on the hygrometric state of the atmosphere, which allows of a more or less speedy evaporation. This is the reason why the numbers given by different travellers, for the temperature of the water on the edge of the basin, agree so little; we ourselves made a series of observations, the terms of which differ considerably among themselves. They are as follow :—

1846.	h. m.		Temperature at the margin.
3d July, at	10 30 A.M. ;	basin filled,	86·0 centigrade.
3d " "	10 58 A.M. ;	"	89·0 "
3d " "	11 30 A.M. ;	"	88·0 "
3d " "	3 25 P.M. ;	"	89·7 "
4th " "	12 35 P.M. ;	"	76·0 "
4th " "	1 17 P.M. ;	"	85·0 "
9th " "	1 50 P.M. ;	basin half full,	82·5 in the wind.
9th " "	1 50 P.M. ;	"	86·0 under the wind.

We will admit that the mean temperature of the water at the surface and in the centre of the basin is 85° C.

The following table indicates the temperatures found in different points of the liquid column, immediately before a great eruption :—

Third Experiment. The 7th July, at 2h. 55m. in the afternoon: four hours before a great eruption. Basin filled: total depth, 23m·50; length of the line, 22m·85.		Heights. m	*Fourth Experiment.* The 7th July, at 6h. 58m. in the afternoon: ten minutes before a great eruption. Basin filled: total depth, 23m·50; length of the line, 22m·85.		Heights. m
	85·0	22·85		85·0	22·85
Therm. No. 1, D	85·2	19·55	Therm. No. 5, D	84·7	19·55
Therm. No. 2, B	106·4	14·75	Therm. No. 1, B	110·0	14·75
Therm. No. 5, D	120·4	9·85	Therm. No. 6, D	121·8	9·85
Therm. No. 6, D	123·0	5·00	Therm. No. 1, D without result.		5·00
Therm. No. 1, B	127·5	0·30	Therm. No. 2, B	126·5	0·30
		Bottom.			Bottom.
Mean temperature of the column of water, 108°·33.			Mean temperature of the column of water, 109°·19.		

The mean temperature of the column of water was obtained by multiplying half the sum of the indications of two consecutive thermometers, by the interval which separates them, and dividing the sum of all these products by the sum of the intervals, or the length of the line.

Twelve minutes after the fourth experiment, at $7^h\ 40^m$ in the evening, the great eruption took place.

The following are the results of the fifth experiment, commenced the 7th July, at $9^h\ 45^m$ in the evening, two hours after the great eruption; basin half-filled; height of the liquid column, $22^m{\cdot}75$; length of the line, $22^m{\cdot}50$.

	°	Heights. m
	85·0	22·50
Thermometer No. 1, D . . .	103·0	13·50
Thermometer No. 3, D . . .	121·0	9·70
Thermometer No. 5, D . . .	lost	4·90
Thermometer No. 6, D . . .	122·5	0·30
		Bottom.

Mean temperature of the column of water, 108°·83.

The three preceding tables show, in an evident manner, that the mean temperature of the column of water which fills the central well of the Great Geysir, remaining in other respects constant, the distribution of the temperature, in different points of that column, varies with the moment when the observation was made. We see, on examining these tables, that there is at the bottom of the well a maximum, immediately before the eruptions, which may be estimated at 127°; and a minimum, immediately after, which may be estimated at 123°.

The results given in the following tables, obtained at different periods between two consecutive eruptions, show that the phenomena always pass in the same manner, and with tolerable regularity:—

First Experiment.

The 6th July, at 0h. 30m. in the morning: three hours after a great eruption, and eleven hours before the following eruption. Basin half-full: height of the liquid column, $22^m{\cdot}75$.

	°	Heights. m
	85·0	22·50
Therm. No. 5, D . . .	95·0	19·70
Therm. No. 3, D . . .	109·0	16·30
No result.		12·90
Therm. No. 2, B . . .	121·1	9·50
Therm. No. 1, B . . .	121·6	6·00
Corresponding number of the fifth exp.	122·5	0·30
		Bottom.

Mean temperature of the column of water, 112°·68.

Second Experiment.

The 6th July, at 8h. 20m. in the morning: nine hours after a great eruption, and twenty-three hours before the following eruption. Basin filled: height of the column, $23^m{\cdot}50$; length of the line, $22^m{\cdot}85$.

	°	Heights. m
	85·0	22·85
Therm. No. 6, D . . .	82·6	19·20
Therm. No. 5, D . . .	85·8	14·40
Therm. No. 2, B . . .	113·0	9·60
Therm. No. 3, D . . .	122·7	4·80
Therm. No. 1, B . . .	123·6	0·30
		Bottom.

Mean temperature of the column of water, 102°·30.

Thus, notwithstanding the relative lightness of the most heated water, which tends to rise to the surface, we see that, even after the eruptions, the temperature of the different strata does not become equalized; still further, in proportion as the inferior strata become heated, to reach their maximum, the upper strata become cool, undoubtedly in consequence of evaporation, and of the heat communicated to the sides of

the well and the basin; so that the greatest difference of temperature at the two extreme levels corresponds precisely to the moment of the eruptions.

We can now ascertain what would be the boiling point of water submitted to a pressure equal to that of the atmosphere, augmented by the liquid column, the height and mean temperature of which we have determined in our five experiments. For this purpose, we must first express the pressure of this liquid column in millimetres of mercury at 0°. The density of the water of the Geysir, found at from 1·0010 to 10° centigrade; the curve which represents the results obtained by M. Isidore Pierre for the dilatation of distilled water, and which I admit as being applicable to the water of the Geysir, extending it up to 113°; lastly, the density of the mercury at 0°, compared to that of distilled water at 4°, and found at 13·596 by M. Regnault, enables us to make this reduction. We thus find that the heights of the columns of water in our different experiments correspond, in millimetres of mercury at 0°, to the following numbers:—

	m
Third and fourth experiment, ...	1·645
Fifth experiment, ...	1·593
First experiment, ...	1·587
Second experiment, ...	1·654

From the equations

$$\log e = a - b\alpha^x, \qquad x = t - 100°,$$

and from the data

$$\log \alpha = \bar{1}\cdot 9977641, \qquad \log b = 0\cdot 4692291, \qquad a = 5\cdot 8267990,$$

laid down by M. Regnault, in which e represents the tension of steam at saturation, expressed in millimetres of mercury, and x the temperature to which that pressure corresponds, we can derive the value of x and of t; and this last quantity will precisely represent the boiling point of water submitted to the pressures.

$$0\cdot 760 + 1^{m}\cdot 645, \qquad 0\cdot 760 + 1^{m}\cdot 593,$$
$$0\cdot 760 + 1^{m}\cdot 587, \qquad 0\cdot 760 + 1^{m}\cdot 654,$$

The corresponding values of t are as follow:—

Third and fourth experiment, . . .	$t = 136°\cdot 151$
Maximum temperature at the bottom, .	127
Difference, ...	9°·151
Fifth experiment, ...	$t = 135°\cdot 398$
Minimum temperature at the bottom, .	122°·5
Difference, ...	12°·898
First experiment, ...	$t = 135°\cdot 31$
Temperature at the bottom, . . .	122°·5
Difference, ...	12°·81
Second experiment, ...	$t = 136°\cdot 28$
Temperature at the bottom, . . .	123°·60
Difference, ...	12°·68

Thus, at the point to which the lower thermometer can penetrate, the water does not attain the temperature of ebullition which it should have under the pressure to which it is subjected; but it approaches that temperature in proportion as a great eruption is near. We may thence conclude that the centre of heat which raises the temperature of the water of the Geysir, is situated at a certain distance below the presumed bottom of the central well, and we may deduce from this a plausible explanation of the manner in which the eruptions are determined.

Let us in fact suppose that the column of water in the central basin communicates, by a long and sinuous channel, with the space, be it what it may, which receives the direct action of the subterranean heat: after an eruption, during which a projection of a great quantity of water and vapor has taken place, the lower parts of the liquid mass are cooled, and the steam which is formed in the reservoir, submitted to the action of the heat, has a less tension than that at which the weight of the central column and that of the atmosphere are in equilibrium; this vapor, as it forms, becomes condensed in contact with the water which fills the sinuous channel, and it imparts to that water its latent heat. The increase of temperature of the water of the channel is transmitted by degrees to the lower part of the central column, where the thermometer can reach; but this increase is retarded by the atmospheric air and the other gases which accompany the vapor; however, at the lapse of a shorter or longer time, the water in the channel must boil, and the steam which continues to form, cannot longer condense there; this vapor must therefore accumulate, and acquire a gradually increasing tension, until this tension is able to overcome the resistance of the column of water which fills the basin, and to project it into the air.

A thermometer placed at four metres above the bottom of the central well, during an eruption which took place July 14, 1846, at $3^h\ 15^m$ in the morning, and which raised the column of water to $49^m{\cdot}37$, according to a measurement taken by M. Waltershausen, indicated a temperature of $124°{\cdot}24$, which corresponds perfectly to what would have been found at the same point in the third and fourth experiments.

In the intervals of the great eruptions it often happens that the column of water is raised to one metre, or $1^m{\cdot}50$ above the usual level, and it is easy to conceive that it must be thus; for if the vapor formed in the subterranean reservoir, is not condensed entirely at its contact with the water of the sinuous channel, of which I have spoken, and if it is accompanied with a sufficient quantity of atmospheric air, or of other gases, it must in the end divide the liquid column, and escape at the surface, producing those subterranean detonations and those jets which all travellers have remarked.

11. *On the Fossil Botany and Geology of the Rocks associated with the Coal of Australia;* by Frederick M'Coy. With 9 plates. (Ann. and Mag. Nat. Hist., vol. xx, pp. 145, 226, 298.)—We cannot in this place better present the results of Mr. M'Coy's very valuable investigations, than in his own concluding remarks.

"Having far exceeded the limits I had originally intended for the preceding part of this paper, I find it only possible to give a brief outline of those general topics on which I intended to have dwelt. First, as

to what has been already done: geologists are familiar, from the labors of M. de Strzelecki and others, with the fact that there exists a series of stratified deposits, consisting of siliceous and argillaceous slates, limestones and sandstones, stretching at irregular intervals from the Liverpool range of mountains in New South Wales to the extremity of Van Diemen's Land, and forming detached masses, probably at one period continuous; these contain abundant fossil remains of animals referable to the palæozoic period.

Above these we have a series of clays, shales and sandstones, with remains of fossil plants and beds of coal, occupying three great basin-shaped hollows; one in the district about the Hawkesbury [Hunter?] River in New South Wales, and called the Newcastle basin, and the two others in Van Diemen's Land, called respectively the South Esk and the Jerusalem basins.* The animal beds containing the palæozoic remains are found, with one doubtful exception, to dip constantly under the coal-bearing strata, at every point of observation; for the most part at the same angle as that at which the coal crops out: the exception alluded to is a point near Spring Hill, Van Diemen's Land, where masses of clay containing *Pachydomus globosus* seem to rest on a sandstone containing remains of plants, and which is known to belong to the top of the coal series: Count Strzelecki, who made this observation, doubts its correctness himself, and expressly states that it needs re-examination to establish the fact of those *Pachydomus* clays really existing in this position. Nevertheless the inference has been drawn from this observation, that the Jerusalem coal-basin was much older than that at Newcastle, N. S. Wales, where the sandstone containing the *Pachydomi* were always seen to dip distinctly under the coal-measures: countenance was apparently given to this supposition by the few plants which were collected from Jerusalem coal-field proving to be all specifically and some generically distinct from those known to exist in the Newcastle basin. When to this we add, that the beds containing the fossil animal remains rest on a siliceous breccia, the age of which is unknown, and that the coal strata are overlaid by variegated sandstone and yellow limestone, supposed from its few organic remains to belong to the pleiocene period, we have I believe stated all that is known on the geological relation of those deposits.

With regard to their palæontology,† we have seven species of plants noticed in M. de Strzelecki's work by Mr. Morris, one of which is identical with a species from the Indian coal-field of Burdwan; and the general resemblance to the oolitic plants of Britain is noticed, as well as the absence of the characteristic forms of the older genuine coal-fields of Europe. In the inferior strata there are forty-eight species of animal remains noticed, one of which is supposed to be identical with a species of the British mountain limestone; the remainder are all (except two new types) of palæozoic genera; but the absence is remarked of *Nautilus*, the true *Leptænæ* and *Orthidæ*, all of which however I have now been able to add.

* The Illawarra coal region is a hundred miles south of Newcastle on the coast, and is properly a distinct district.—J. D. D.

† In a note, Mr. M'Coy here alludes to the Report on the Geology and Palæontology of this district, by Mr. Dana, now ready for the press, of which he was recently informed by letter.

In the above notice I have given seventeen species of fossil plants from the Mulubimba district, which is a portion of the great Newcastle and Hawkesbury basin, twelve of which are considered new. Those plants belong to ten genera, two of which (*Vertebraria and Zeugophyllites*) are only known here and in the supposed oolitic coal-fields of India: one genus (*Gleichenites*) I have provisionally used for the *Pecopteris odontopteroides* of Morris, from the verbal characters given by Göppert for that genus, the species of which are found only in the palæozoic coal; the plant however agrees much better with the species of the Keuper genus *Heptacarpus* than with those of the carboniferous *Gleichenites*, and if we look rather to the plants themselves than to the definitions given of the genera, I should certainly place it there; all the other genera (with the exception of *Phyllotheca*, which is confined to the locality) are well-known in the oolitic coal deposits of Yorkshire, and one species, the *Sphenopteris germana* (M'Coy), is scarcely to be distinguished from the common *Pecopteris Murrayana* (Br.) of the Scarborough shales. Several of those genera are common both to the carboniferous and oolitic periods, but the most abundant and characteristic plants of the Australian beds belong to a genus (*Glossopteris*) never found in the old coal-fields, but several species of which are, on the other hand, well-known in coal-beds of the oolitic age in various parts of the world. I am therefore strongly of opinion, from the evidence of more than double the number of species of plants known before, that the coal deposits of Australia should be referred to the oolitic period; and this opinion derives much additional weight from the negative fact, that among the large quantity of remains of plants which I have examined from this district, not a trace has been observed of any of the characteristic carboniferous genera—not a trace of *Lepidodendron* or any allied plant—not a trace of *Sigillaria*, *Favularia*, *Stigmaria*, or even of true *Calamites*. I might further add, that the list of plants I have given destroys any negative arguments formerly based on the fossil evidence, for considering the Jerusalem coal-basin to be of a different age from the Newcastle one, as I have detected the most characteristic plants of the former abundantly in the latter beds, so that the fossil evidence now would go, with the admitted identity of the walls of the basins and the general analogy of the sections, to prove them all of one age.

In the underlying rocks I have been able to determine 83 species of animal remains, of which 14 are *Zoophyta*, 3 *Crinoidea*, 4 *Crustacea*, 25 *Brachiopoda*, 24 *Lamellibranchiata*, 6 *Gasteropodo*, 4 *Pteropoda* and 3 *Cephalopoda* (including *Bellerophon*). Of these, 4 genera and 32 species are figured and described as new. Those 83 species belong to 39 genera, all of which (with the exception of the genera *Tribrachyocrinus*, *Pachydomus*, *Notomya*, and *Eurydesma*,—new forms at present only known in Australia) are abundant in the carboniferous rocks of Britain, many of them not being found in any higher series, and several of them not being known in any older deposits, so that the age, even if *we only look to the genera of fossils*, is clearly limited to the carboniferous periods; but when we descend to the critical examination of species, we find so extraordinary and unexpected an amount of agreement between those beds and the similar shales, sandstones and impure lime-

stones forming the base of the carboniferous system in Ireland, that it is impossible not to believe them to be nearly on the same parallel, and there is equal difficulty in imagining them to be either younger or older than those deposits. Of those species no less than eleven are believed to be positively identical, on the most careful comparison of the Australian and Irish specimens, and nine more are so closely allied that it has been found impossible to detect any difference of character, but which, either from imperfect preservation or want of sufficient specimens to display all the characters, have not been specifically identified. With such evidence as I have mentioned, I do not think it improbable that a wide geological interval occurred between the consolidation of the fossiliferous beds which underlie the coal, and the deposition of the coal-measures themselves; that there is no real connexion between them, but that they belong to widely different geological systems, the former referable to the base of the carboniferous system, the latter to the oolitic, and neither showing the slightest tendency to a confusion of type."

III. Zoology.

1. *On a new genus and species of Fossil Ruminantia: Poëbrotherium Wilsoni;* by Joseph Leidy, M.D., (Proceed. Acad. Nat. Sci., Philad., Nov., 1847, p. 322.)—Indirectly through Mr. J. S. Phillips and the influence of Dr. S. G. Morton, the Academy has become the depository of a valuable and unique fossil, received through Dr. S. D. Culbertson of Chambersburg, Pa., from Mr. Joseph Culbertson.

As first received, it consisted of a mass of argillaceous limestone, having one side of a cranium of an animal exposed to view, which, by the patience of Dr. T. A. Wilson, was relieved of its matrix, and the lower extremity of the humerus, and the upper extremity of the ulna and radius of the right leg were also disclosed.

The top or vault above the orbits, and posterior part of the cranium, are wanting, as are also the ossa nasi, ossa intermaxillaria, the part of the os maxillare inferius just anterior to the commencement of the symphisis, and the zygoma of the left side, but sufficient is left to characterize it as a remarkable genus of *Ruminantia*, very different from any that has been heretofore described.

The cranium belonged to a full grown or adult animal, but not an old one, as is indicated by the teeth.

In the upper jaw are seven molars, differing in this respect from any ruminant known, living or fossil. The posterior three molars, usually called true, present nothing very peculiar in their conformation. They are not so square as in *Cervus*, but are more like those of *Oris*, being much broader than wide, so that they have a compressed appearance. The four crescents upon the crowns are quite simple. Externally these teeth present two and nearly plane surfaces, separated by an abrupt, salient, longitudinal ridge on a line with the notch separating the anterior and posterior pair of columns. Each of these surfaces has a longitudinal rounded ridge, more prominent upon the anterior than the posterior one, but neither so salient as the first. The antero-external border is also elevated or prominent, so that each of these teeth presents externally four longitudinal ridges. As is usual, these teeth are

obliquely situated in the jaw, and the anterior part of one folds over externally or overlaps the posterior part of the one preceding it.

The anterior four molars or premolars are not more than half the length of the true molars, and differ among themselves so as to render it necessary to examine them separately. The posterior or fourth premolar has more the characteristics of a true molar, and it would probably not be wrong to consider it as an additional true molar. The crown presents four crescents, which are thicker than in the true molars, and the anterior and posterior pair are separated by a comparatively deeper notch. Externally the tooth has four ridges corresponding to those of the true molars. The third premolar, or the one immediately preceding the last, has upon its crown a posterior pair of thick crescents, and an anterior cusp which has the appearance of being formed by the blending together of a pair of crescents. Externally it is trilobed, the lobes being separated by two concave depressions. It is shorter but broader than the last. The second premolar is compressed, faintly trilobed, and presents an elongated trenchant crown. The first premolar is the most remarkable characteristic of this cranium. It is separated from the others by a concave notch of ·333 of an inch, and is on a line with the anterior mental foramen. It is implanted in the jaw by two fangs, which are divergent and placed one anterior to the other. The body is nearly as broad as the second premolar and is of a compressed pyramidal form, and the crown has a trenchant edge, the posterior and anterior part of which form an angle about its centre.

In the lower jaw, in the specimen, are six inferior molars in a closed row commencing ·25 of an inch anterior to the corresponding six molars above, and continuing as far back as the latter. Besides these, and separated from them by a concave, descending notch of ·45 of an inch, just anterior to the anterior mental foramen, or ·15 of an inch anterior to the commencement of the symphisis posteriorly, is one half of an alveolus for an additional or seventh molar, which, when the specimen was first received, contained a portion of a fang, since mislaid. This additional molar in the lower jaw, is possessed by only one other known genus of *Ruminantia;* the *Dorcatherium*, of Kaup.

The crowns of the inferior molars are enveloped in the matrix in such a manner that they cannot be exposed without endangering the specimen. Externally the three true molars present their columns as sharply triangular prisms, as in *Ovis*, &c., and have no intervening points or cones, as in *Cervus*, *Dorcatherium*, &c.

The fourth premolar is trilobed externally, each lobe presenting a cusp towards the crown. The third and second are compressed, and the latter, I can perceive, has a trenchant crown.

The position of the molars, though resembling that of *Dorcatherium* considerably more than that of any other genus of *Ruminantia*, differs materially from it, for while the teeth reach to the symphisis in the latter, in the former they even extend anteriorly to its commencement.

From the foregoing description of the teeth, it will be perceived, that in the possession of a seventh molar in the upper jaw, in the position of the molars, and in several other minor peculiarities, this genus

differs from all others heretofore known, and is well characterized, and I therefore propose for it the name of "*Poëbrotherium*."*

[We omit some farther details, and pass to the concluding remarks.]

These bones belonged to an animal, rather less in size than the *Dorcatherium*.

The species I have designated *Wilsoni*, in honor of Dr. Thomas B. Wilson, a munificent patron of the natural sciences.

Probable habit of the animal.—From the evidences of considerable muscular strength in the posterior part of the inferior maxilla and the trenchant crowns of the anterior premolars, it might be supposed that the animal was adapted to eating flesh as part of its food, as was thought by Cuvier to have been probably the case with the *Anoplotherium gracile*, a pachydermous animal having very similar characters; but I should think its general structure would entirely preclude the idea of its having been able to catch living animal prey, and doubt very much whether its food could have been other than vegetable. The anterior trenchant molars were more probably intended for cutting branches and twigs of bushes, or tough grasses, which afterwards underwent a finer trituration with the true molars.

The position which the genus should occupy.—*Poëbrotherium* in its dentition approaches the *Ruminantia* to the *Pachydermata*, for in the number of the molar teeth and the trenchant nature of the anterior premolars, it is closely allied to the Xiphodont *Anoplotherium*, while in the true molars it is characteristically Ruminant, and its position would therefore, probably stand thus: *Dorcatherium*, *Poëbrotherium*, *Anoplotherium*.

Measurements† of the head.

	In.
Mentus auditorius externus to infra orbitar foramen, .	3·1
From point of hook-like process of inferior maxilla to anterior mental foramen, ...	4·35
Greatest width of orbit, ...	1·15
Narrowest part of face, below ossa nasi, . . .	·2
Width at the corono-condyloid depressions of inferior maxilla, ...	1·6
Width at the coronoid processes, ...	2·
Greatest width at the ossa tympani, ...	2·1
Distance between ossa tympani, ...	·375
Width of os tympanum, ...	·85
Length of row formed by the posterior six superior molars, ...	2·5
Notch between the first and second superior premolars,	·333
Length of row formed by the posterior six inferior molars, ...	2·7
Notch between the first and second inferior premolars,	·45

* πόα, *herba*, βρόω, *pasco*, θήρ, *fera*.

† The measurements are taken in English inches and parts of the same.

Measurements of superior molar teeth.

	Length.	Breadth.	Thickness.
7th molar, . . .	·375	·6	·2
6th " . . .	·4	·55	·25
5th " . . .	·333	·45	·275
4th " . . .	·2	·375	·25
3rd " . . .	·15	·4	·2
2nd " . . .	·1	·35	·1
1st " . . .	·15	·3	·075

Measurements of inferior molar teeth.

	Length.	Breadth.
7th molar, . . .	·3	·35
6th " . . .	·3	·5
5th " . . .	·25	·4
4th " . . .	·15	·45
3rd " . . .	·1	·35
2nd " . . .	·1	·35

Measurements from bones of fore-leg.

Transverse diameter of lower articular surface of os humeri, ·75
Antero-posterior diameter in depressed portion of same, ·45
Length of olecranon above the lowest part of the articular surface of the elbow, ... ·95

2. *Description of a new species of Antelope from West Africa;* by J. E. Gray, Esq., F.R.S., (Ann. Mag. Nat. Hist., Oct. 1847, vol. xx, p. 286.)—Mr. Whitfield last year brought with him from the Gambia, along with other most interesting mammalia and birds, some horns of a very large species of Antelope, called by the natives *Gingi-ganga*, which very nearly resemble those of the *Eland* from South Africa, but are larger, longer and much heavier than those of the large male *Eland* from South Africa, which the Earl of Derby presented to the British Museum on the return of Mr. Burke.

This season Mr. Whitfield succeeded in procuring the upper part of the skull and horns of a male, and the flat skin (but unfortunately without head or feet) of an adult male and female of this animal, which proves to be perfectly distinct from the Cape species; and as it is by far the finest Antelope known, I propose to dedicate it to the Earl of Derby, who has done so much to illustrate the species of this group, and has been so successful in importing and breeding the various kinds.

This species is distinguished from the *Cape Eland* by the neck and front part of the under side, and a large spot on the front and hinder side of the upper part of the fore-legs (and the fetlock) as well as the dorsal line being black, and by the side being ornamented with fourteen or fifteen narrow, rather waved, perpendicular white lines, and the lower part of the neck nearly surrounded with a broad white half-collar which narrows above.

The species may be thus described.

Boselaphus Derbianus. The Black-necked Eland or *Gingi-ganga.*

Pale reddish brown; neck, front part of the under side, the dorsal line, a spot on the front and hinder part of the upper part of the foreleg ("and fetlock") black; broad half-collar on lower part of the neck, and fourteen or fifteen narrow perpendicular lines on each side of the body white; belly and front and hinder side of thighs whitish; crown reddish brown; withers variegated with black hairs.

Female? Neck blackish brown; rest like male.

Inhab. Western Africa, Gambia.

3. *On a Mode of Reproduction in the Inferior Animals;* by J. P. VAN BENEDEN,* (Acad. Sci. Brussels; from the Institut, No. 718.)—The close affinity between the three groups of minute polyps, Campanularia, Tubularia, and Sertularia, is well known. In the first two groups, the species produce a medusa-shaped young; van Beneden has directed his investigations towards determining whether this is also true of the Sertularidæ. His observations were made on the *Thoa halecina* and *Sertularia cupressina*, of the Brussels coast. He thus concludes from his study.

The Tubularidæ, Campanularidæ, and the Medusæ, and *perhaps* the Sertularidæ, pass through the same phases of development. From the egg proceeds a ciliated larve; this larve multiplies by buds and stolons, and so produces a compound group or zoophyte. Then some portion of this common mass or colony, becomes the seat of a peculiar kind of reproduction—a reproduction by "scission;" in the Medusæ the *strobile* form is produced; while in the polyps ovarian sacs are formed containing movable embryos. In both cases the animal separates from the colony under a new form; it swims at large, and produces eggs.

As the medusa phase is not essential in polyps, so the polyp phase is not necessary for medusas; and there may be several successive generations sometimes of one, and sometimes of the other exclusively; that is, we may have medusas giving birth to medusas without going through the form of a polyp, and polyps producing polyps which have not passed through the intermediate form of the medusa.

The mode of reproduction in the *Salpa*, seems not to have been fully understood, because it has been supposed that isolated individuals multiply in the same manner as those aggregated in chains. But this is not so. The former produce buds, the others eggs. The Salpa

* M. J. P. van Beneden is the author of various valuable and splendidly illustrated memoirs, the result of vast labor and minute investigation. They are principally anatomical and physiological examinations of different mollusca, and minute polyps, and have mostly appeared in the Transactions of the Brussels Royal Academy. The following are the titles of his more recent memoirs, relating to Polyps, Bryozoa and Ascidia.

Histoire Naturelle des polypes composés d'eau douce.—Ibid, vol. xvi, 1843.

Memoire sur les Campanulaires de la Côte d'Ostende, considérés sous le rapport physiologique, embryogenique et zoologique.—Mem. Acad. Roy. Brux., xvii, 1844.

Recherches sur l'embryogénie des Tubulaires, et l'histoire naturelle des differents genres de cette famille qui habitent la côte d'Ostende.—Ibid, vol. xvii, 1844.

Recherches sur l'organization des *Laguncula*, et l'histoire naturelle des differents Polypes Bryozoaires qui habitent la côte d'Ostende.—Ibid, vol. xviii, 1845.

Recherches sur l'anatomie, la physiologie, et le développement des Bryozoaires qui habitent la côte d'Ostende.—Ibid, vol. xviii, and xix, 1845.

Recherches sur l'embryogenie, l'anatomie et la physiologie des Ascidies simples.—Ibid, vol. xx, 1847.

proceeding from an egg gives birth to several buds which remain together and form a chain. These last then produce eggs whence come isolated embryos; all the phenomena of reproduction in the Salpa may be compared to the young tree growing from the seed and giving birth to buds.

Reproduction in the *Tænia* and *Bothrocephalus* appears to be similar to that in the *Salpa*, with this difference that the first generation growing from the eggs has a different form, as in the Medusæ, &c., from the second proceeding from the buds. The rings of these worms, or the articulations, separate and represent the last phase of development; and as in the Medusæ and Salpa, it is in this generation that the sexual development takes place.

The Hydra, and several Annelida present a similar phenomenon. Müller long since made known the fact of a reproduction by buds in the *Nais;* Milne Edwards has observed the same in the *Myrianida fasciata* (Ann. Sci. Nat., March, 1845); Sars has given another example in the *Filagrana implexa.* As in the preceding case, it is only after several generations by buds that the sexual generation appears. The individuals proceeding from buds and from eggs are here more or less similar in external form; those producing buds are peculiar in having no sex. After this comparison, the pretended anomalies disappear.

In the simple Ascidia, generation takes place by metamorphosis direct. But in the compound Ascidiæ, it does not appear to be so. Indeed Sars has shown that the larve gives origin to the compound Ascidiæ, which in mass become fixed. It can scarcely be doubted that the larva state in which the animal has organs of sense, represents the polyp phase of the medusa.

The Ascidia enables us then to understand the remarkable mode of development explained by Steenstrup, in the Cercaria, which changes after some generations into a Distoma. There is in these animals a gemmiparous generation; but as in the Ascidia, in place of the buds developing outward, they are organized within, and the mother seems to serve only as a case for the generation following. It is this which led Leblond to say, not understanding this phenomenon, that he had found an entozoum within an entozoum.

There is in the first an *exogenous gemmiparism*, and in the last an *endogenous gemmiparism.* No doubt we shall soon find other facts of the same kind which will show still better the character of these different phenomena. We have thus some generations which resemble their immediate parents, and some generations which resemble only the preceding parents. It is this phenomenon which is so striking in the Salpa and which has given rise to the term *alternating generation.*

We may conclude therefore by observing, that these animals present two modes of reproduction, one by eggs and the other by buds, and that the genetic evolutions pass through different phases in the two cases. One of these generations may be called *oogenous* and the other *phytogenous.* There is a series of metamorphoses for those which proceed from an egg, and there is another for those which grow by buds. The first alone is analogous to that of the superior animals. It is not then by the term *alternation* that we express the fundamental character of these remarkable reproductions.

IV. Astronomy.

1. *Satellite of Neptune.*—M. Otto Struve, in a letter to M. Le Verrier, an extract of which is published in the *Comptes Rendus*, Nov. 29, 1847, states, that soon after the remounting of the grand refractor at Pulkova, he directed it (for the first time during the year 1847) to the planet Neptune, and at once recognized the satellite. This was discovered a year previous by Mr. Lassell of Liverpool. Mr. Struve obtained several measurements of the angle of position, and the distance of the satellite from the primary, viz., six in September, two in October, and one in November. From these observations, he derives the following elements of the orbit, viz.

Passage of asc. node,	1847, Sept. 27·0 m. t. Pulkova,
Period of revolution, - - -	5d. 21h. 15m.
Radius of orbit, - - - -	17″·89
Longitude of asc. node, - - -	119° or 357°
Inclination, - - - -	34°·8

The mass of Neptune deduced by Mr. Struve from this semi-axis major and period of revolution, is $\frac{1}{14494}$th of the mass of the Sun;—a value which he considers quite exact; and probably more accurate than the usually received mass of Uranus.

In regard to the supposed ring around Neptune, he states, that although he has examined the planet on favorable nights, he cannot detect any trace of such an appendage.

At a session of the American Academy, in December, 1847, "Prof. Peirce communicated the following elements of the orbit of the satellite of Neptune, computed from the combination of all of Lassell's and Mr. Bond's observations; and he also communicated the corresponding mass of the primary.

"Time of sidereal revolution,	5 days 21 hours 12·4 minutes.
Inclination to ecliptic, - -	29°·9
Longitude of asc. node, (the motion being supposed direct,)	119°·8
Time of greatest northern elongation,	Nov. 26·53 Greenw. m. s. t.
Greatest elongation, - -	16″·5
Distance of satellite from Neptune,	230,000 miles.
Corresponding mass of Neptune, $\frac{1}{13133}$, the mass of the Sun being 1."	

2. *Orbit of the Planet Flora*, (Comptes Rendus, Dec. 6, 1847.)—The following elements of the orbit of the planet Flora, discovered Oct. 18, 1847, by Mr. J. R. Hind of London, were computed by Mr. Hugh Breen, Jr., assistant in the Royal Observatory at Greenwich. The first set is derived from observations of Oct. 21, and Nov. 1, and 10;—the second from observations of Oct. 21, and Nov. 9, and 23, allowing for parallax and aberration:

	1st Elements. App. Eqx. Nov. 10.	2d Elements. True Eqx. Nov. 10.
Epoch, m. t. Greenwich,	Nov. 10, 563183	Oct. 21, 622026
Mean anomaly,	18° 14′ 8″·94	6° 34′ 49″·21
Long. of perihelion,	36 28 57 ·96	44 29 29 ·97
" " asc. node,	109 45 58 ·13	109 4 44 ·58
Inclination,	5 54 46 ·10	6 4 6 ·01
Angle of excentricity,	8 11 48	7 20 38 ·33
Mean daily motion,	0 18 30 ·062	0 18 26 ·92
Logar. of semi-axis major,	0·3364396	0·33726 03

3. *On the Annular Eclipse of the Sun, October*, 1847; by J. Lawrence Smith, (in a letter to the Editors.)—I doubt not but that you have heard much concerning the annular eclipse of the sun seen in Europe and Asia on the 9th of Oct., 1847, and therefore should not address you on the subject, were it not that any item of information connected with this form of solar eclipse must be of interest to you. In the western part of Europe, the eclipse took place so early in the morning as to render all observations there very imperfect, except the atmosphere were unusually clear; it was therefore in the east of Europe and in Asia, that we were to look for it in all its beauty. Unfortunately at this place (Constantinople), the morning was hazy and cloudy; the clouds breaking sufficiently from time to time, to furnish a view of the eclipse; in fact with the small telescope I used, all the times of the eclipse were made out within two or three seconds. The commencement of the eclipse, although seen but imperfectly, was strikingly beautiful; without the slightest appearance of the proximity of any body to the sun, its western limb became suddenly obscured by a black tache; the contact took place at 24° west of the highest point of the sun, at $8^h\ 16^m\ 08^s$. After the eclipse had commenced, the atmosphere became more and more favorable for its observation, and before its conclusion, the sun shone with almost its usual brilliancy. The ring commenced to be formed, $9^h\ 44^m\ 10^s$. The separation of the borders of the sun and moon, was perhaps the most interesting part of the phenomenon; the separation was instantaneous, and nothing but an *uninterrupted* circle of light was seen, gradually becoming more and more uniform in its breadth, its outer border being perfectly circular, while the inner was more or less indented by the projection of the lunar mountains upon the disk of the sun. At $9^h\ 48^m\ 02^s$, the centres of the sun and moon were at their nearest proximity, that of the moon being only $2\frac{3}{4}''$ above that of the sun; in fact so near were they, that the ring appeared uniform. The ring commenced to break at $9^h\ 51^m\ 52^s$, presenting all the phenomena of its formation in an inverse order. The moon as it passed the border of the sun, appeared to have on its outer edge several brilliant spots, caused by the passage of the light between the lunar mountains. At $11^h\ 30^m\ 28^s$, the eclipse had entirely passed away, its duration having been $3^h\ 14^m\ 21^s$; that of the ring was $7^m\ 42^s$.

The light of the sun was not greatly diminished; in fact after the eclipse had been three-fourths formed, there was no sensible diminution of the light. When the ring was formed, it represented about one-seventh of the solar surface. I should have examined the changes of light and heat, but the weather was not sufficiently favorable for this purpose. I come now to say one word about a phenomenon which, although not an astronomer as you well know, interests me in common with all men of science; it is the formation of black streaks immediately as the western limb of the moon becomes detached from the western limb of the sun, and just before the eastern limbs touch; that is to say, when the border of the lunar mountains has quit that of the sun, a continuous ring of light is not seen, but parallel black streaks seem to reach from one star to the other, which ultimately break as the moon advances. These streaks are said to occur as "if the two stars were fastened together by a glutinous matter, adhering to certain points of the sun, the

ligaments of which are distended and finally snapped asunder by the motion of the moon. On the approximation of the eastern limb of the sun, the phenomenon reappears in the inverse order, and the black and parallel lines are the first to appear instantly."

This phenomenon is one that is far from being generally admitted as properly belonging to the eclipse. M. Arago attributes it to bad adjustment of the telescope. Mr. Baily in Scotland witnessed it during the annular eclipse of May 15, 1836; but it was not verified by the total eclipse of July 8th, 1842. In the case of the present eclipse, it was noticed by Signor Gallo of Trieste, and M. Capocci of the same place, the former with a twenty-eight inch refracting telescope, of the manufacture of Kohlgrub in Bavaria, and the latter with a forty-inch refractor, made by Benchi of Paris. To use the words of Signor Gallo, "Both of us, without neglecting the customary observations, devoted our special attention to the above mentioned phenomenon, and verified it with signal success in all its detail, at the moment of the rupture of the ring; and although the agitation of the air made the limbs of the two luminaries appear somewhat confused and oscillating, we distinctly discovered, and in great numbers, the mysterious obscure lines, which appeared to us most subtle and fugitive between the oscillation of the limbs." So much then for the observations of some persons on this subject, which are not confirmed by mine, in which however very little weight is to be placed, as the atmosphere was not sufficiently clear for an observation of the kind. A friend of mine however, a Mr. Sang, an astronomer accustomed to these observations, also saw the ring perfectly free from these lines, and he informs me that this is the third he has observed, and always with the same result; the examinations of the two first were with a good reflector.

With these conflicting evidences concerning a phenomenon so interesting if true, we can form at best but an uncertain opinion. As for myself, I do not think that it really belongs to the eclipse, but that it arises from imperfections in the glasses of the telescope, which, if free from them, will exhibit the projection of the moon upon the sun clearly and distinctly, the borders of the mountains being well defined. Those who are familiar with the manner in which telescopic lenses are prepared, know that the maker desires to polish them as little as possible, that is, he tries to bring the glass to its proper finish on the first instrument with emery, of different degrees of fineness, from the fact that the polishing tool, if used too much, may alter the form of the lens, it not being of the same resisting nature as the emery tool; the consequence of this is, that even the best lenses frequently contain what are called emery pits, little cavities invisible to the eye—ordinarily they offer no disadvantage. Now there is no difficulty in supposing that several of these pits occurring together, may form a network capable of giving rise to the black *rays* of Fraunhofer, which in all probability may be the solution of the phenomenon alluded to in the eclipse. This suggestion may have been already made by others; if so, it has not come to my knowledge. Be it probable or not, much precaution ought to be observed before admitting that the phenomenon arises from the action of the borders of the moon, or from any gaseous matter that may surround it. I should prefer establishing it with a reflector than a refrac-

tor, because in the object glass of the latter, there are four surfaces liable to the defects already alluded to, while in the reflector of the former there is but one surface. These facts and opinions I give with much reserve, as the observation of astronomical phenomena is quite out of my sphere of research, which is altogether terrestrial.

During the eclipse, constant firing of cannon was heard in every direction, which arises from superstition among the Turks of the evil tendency of eclipses, and the efficacy that artillery has in dispersing it. Of course among the intelligent of the nation this belief has passed away.

Constantinople, Nov. 30th, 1847.

4. *Meteorite of July* 14, 1847.—A meteorite exploded on the 14th July, 1847, and threw down two or more fragments at Braunau in Bohemia. One fragment weighed forty-two pounds and a half; the other which weighed thirty pounds penetrated the roof of the house of a laborer, but did no harm to the inmates.

V. Miscellaneous Intelligence.

1. *Association of Geologists and Naturalists.*—We continue in this number the publication of abstracts of articles read at the last meeting at Boston, so far as they have been sent to us by their authors. Of a large number of memoirs, we have yet no notice for our pages. We should state that the closing paragraph of the Proceedings in our last number, page 116, was editorial. The Proceedings will be continued in a following number, if we are favored with other abstracts.

2. *Spencer's Microscopes;* (letter from Prof. Bailey to B. Silliman, Jr.) —I am so much pleased with the excellence of an objective recently made for my microscope by an American artist, Mr. Spencer of Canastota, N. Y., that I wish, if it is not too late, to append a few remarks to those which I made concerning the qualities of the microscope made by the same gentleman for Dr. Gilman.* The object which I have used in testing the powers of quite a number of the best microscopes in the country, is the Navicula hippocampus, for English specimens of which I am indebted to Wm. C. Williamson, Esq., of Manchester, England, an accurate microscopic observer, who sent it to me as "*the* test object par excellence." That it is so considered by the English observers, I know by the testimony of several members of the London Microscopic Society, and indeed it is alluded to in a late address by the President of that Society as one of the most difficult test objects. This object has delicate longitudinal and transverse lines, the former of which are easily resolved by microscopes of ordinary quality, but the latter require good defining power to show them at all. I have employed these lines upon the same individual shell, in examining several imported microscopes made by Chevalier, Oberhauser, Plœssels, Ross and Powell, and I have no hesitation in saying that the lenses made for me by Mr. Spencer are superior to any which I have yet seen, made by the three first named opticians; the superiority consisting in much better defining power, and in a larger angle of aperture. With my own Chevalier microscope, it

* See this Number, page 278. The above letter came to hand after Dr. Gilman's article was in type.—Eds.

is only by considerable management of light that I can begin to see the lines alluded to; another similar instrument by the same maker, would not show them at all. Only one out of three of Oberhauser's which I have examined, showed the lines, and then much trouble was experienced in getting the right illumination. The performance of the Ross and Powell instruments is much better. With those the cross lines as well as the longitudinal ones were easily seen, and without requiring much manœuvering of the mirror. It is with these that I compare the performance of my new objectives recently made by Spencer; and although I am well aware that to test fully their relative merits, the instruments should be placed side by side and tried under the same circumstances, yet I think I may trust to my memory so far as to say, that Spencer's lenses showed me all which I could see by the best English instruments in this country, and with quite as much ease. Indeed there is no difficulty at all in seeing the lines with my new objective, and Spencer has fully redeemed the pledge which he made me, when he in my company first looked at the Navicula hippocampus, "I will make for you," said he, "an objective which will show the lines in spite of the most awkward illumination." This he has done, and not only can the lines be seen in almost any position, but when properly illuminated they are shown so distinctly, that I have found no difficulty in copying them by means of a camera lucida, and I have thus ascertained that the distance between the cross lines referred to, is not more than one forty thousandth of an English inch! Such is the performance of Spencer's lenses in my hands, but the artist says they will do still more, and that by means of those I now own, he has resolved the lines on a sigmoid Navicula which I found in the Croton water of New York—an object to which, as Spencer says, "all other test objects are mere child's play." This animalcule has a shell not more than half the size of that of the individual N. hippocampus above referred to. It is possibly the young of the same species, but as it appears to be uniformly much smaller, and its markings are far more minute, I shall consider it at present as a new species, and dedicate it, by the name of N. Spencerii, to the person who first resolved its lines, by instruments of his own making. This object, which from analogy I judged to have lines resembling those on the N. hippocampus, was sent by me to a friend who possesses one of the best foreign instruments. After giving it a long and careful trial, the gentleman to whom it was sent informed me that he had tried in vain to see the lines in question, and he adds that if Spencer's lenses will show them, they must surpass the best European instruments. Now although I cannot positively state that I have seen the lines with Spencer's lenses, the artist himself has seen them with the same objective; and that I have not yet succeeded, may possibly be due to some defect in the eye-piece (one of Chevalier's) with which I have to combine it. That the lines exist, I should have no doubt from the peculiar appearances I have myself seen, even if we had not the positive testimony of Mr. Spencer that he has seen them clearly and unmistakeably. Lenses which will show these lines, (which cannot be more than one eighty thousandth of an inch apart,) may safely challenge competition with foreign microscopes.

West Point, N. Y., Feb. 4th, 1848.

3. *Antarctic Exploration.*—Captain Ross in his British Antarctic Expedition, makes some unfounded assertions and ungenerous remarks, relative to the American Exploring Expedition, which demand correction. We cite the following extracts from a letter by Captain Wilkes, Commander of the American Expedition, published in the Washington Union, Aug. 12, 1847 :—

In Captain Ross's work, vol. i, p. 116, he says :

"That the Commanders of each of these great national undertakings (D'Urville and Wilkes) should have selected the very place for penetrating to the southward, for the exploration of which they were well aware at the time that the Expedition under my command was expressly preparing, and thereby forestalling our purposes, did certainly greatly surprise me. I should have expected their national pride would have caused them rather to have chosen any other path in the wide field before them, than one thus pointed out, if no higher consideration had power to prevent such an interference.

"They had, however, the unquestionable right to select any point they thought proper, at which to direct their efforts, without considering the embarrassing situation in which their conduct might have placed me. Fortunately, in my instructions, much had been left to my judgment under unforeseen circumstances ; and, impressed with the feeling that England had *ever led* the way of discovery in the southern as well as in the northern regions, I considered it would have been inconsistent with the preëminence she has ever maintained, if we were to follow in the footsteps of the Expedition of any other nation."

Without adverting to the bad taste and egotism of the above extract, and the broad assertions relative to the want of national pride in the commanders of the French and American Expeditions, it will only be necessary in reply to point to dates.

On the first page of his introduction are the following statements :—

"At the eighth meeting of the British Association for the Advancement of Science, held at Newcastle, in August, 1838, the attention of the physical section of that body was directed to the number and importance of desiderata in that great practical branch of science called terrestrial magnetism, by Lieut. Colonel Sabine, of the Royal Artillery ; and a committee was appointed, consisting of Sir John Herschel, Mr. Whewell, (now Master of Trinity College, Cambridge,) Mr. Peacock, (now Dean of Ely,) and Professor Lloyd of Trinity College, Dublin, to represent to her Majesty's government a series of resolutions adopted by the British Association ; and as these resolutions exhibit the general outline of objects sought to be attained, they are inserted here, as pointing out clearly the causes in which the Expedition to the Antarctic regions originated."

This meeting took place on the 20th of August, 1838. The instructions to me, as commander of the Expedition, are dated Navy Department, August 11th, 1838 ; and the paragraph relating to the exploration south is as follows :

"These objects will, it is presumed, occupy you until the last of October ; and when attained, as far as may be possible, you will proceed

to the port of Sydney, where supplies may be obtained. From thence you will make a second attempt to penetrate within the Antarctic region, south of Van Diemen's Land, and as far west as 45° east, or to Enderby's Land, making your rendezvous as you return at Kerguelan's Island, or the Isle of Desolation, as it is now usually denominated, where you will probably arrive by the latter end of March, 1840."

The dates prove conclusively that my instructions were issued from our Navy Department nine days before the body met who first took any steps towards the investigations on magnetism, out of which the English Antarctic Expedition had its birth, and when that Expedition was not yet dreamed of. Captain Ross received his appointment to it *nine months* after these dates. * * *

The publication of Captain Ross's Narrative affords me the opportunity of vindicating the Exploring Expedition and myself, from the unfounded assertion set forth by Captain Ross on his return from his first Antarctic Cruise: that he had sailed over our discoveries, or, in his own words, "had found a clear sea where I had laid down mountainous land."

I had no idea until I saw the engraved chart in Captain Ross's book, what was meant by the "mountainous land" which he refers to. On the original chart from which the tracing was taken that I sent him, there is not the least resemblance to the "mountainous land" Captain Ross speaks of, nor to the representation of it on the one he has had engraved. In the original chart, now in my possession, this mountainous land is only twenty-seven miles in length, whilst the engraved one in Captain Ross's book makes it eighty miles; and it is as much unlike in every other respect. I positively assert that the land as it appears on the chart he has had engraved, could not have been traced from any thing that was in my possession then or since. Captain Ross admits that he was in possession of the publications in Sydney, wherein it was distinctly stated that our discoveries did not extend east of 160 degrees east longitude; consequently there could be no reason for his believing the "mountainous land" to be a part of our discoveries; and what will make it still more evident is, that Ross knew that Bellany had sailed over the position that this "mountainous land" occupied on the chart. Why then did he not say so, instead of vaunting that he had sailed over our discoveries?

On page 293, vol. i, Captain Ross relies upon three grounds to exculpate himself from the imputation of endeavoring to underrate the labors of the American Expedition, as well as to detract from the reputation of myself by promulgating so unfounded a report. They are:

1st. That the true position of Bellany's Islands had been given by himself.

2d. That I had a knowledge of their true position; and

3d. That the land was not laid down in that position on the tracing, and therefore could not possibly have been meant for it.

Captain Ross presumes upon data that were not true. I had no knowledge of the true position of Bellany's Island when I wrote to Captain Ross, and no information respecting it but that derived from Captain Briscoe, who marked the position of it on my chart. I had no written

data whatever respecting its position, and I was quite ignorant of Bellany's name, as well as whether it was a collection of islands or main land; if, therefore, the discoverer had assigned to it its true position, I did not know it. I had no reason then to doubt that Captain Briscoe was correct, as I knew he had been, or then was, in the employ of the Messrs. Enderby, who owned the vessel which the discoverer commanded. The position assigned to Bellany's Land by Captain Briscoe, is in 65° 50′ south, 164° 27′ east, about fifty miles from its true position—much nearer the truth than that of many other islands I have visited during my cruise. * * *

4. *On Shell Cameos*, (Society of Arts, April 21, 1847, Jameson's Jour., July 1847, Athenæum, No. 1018, p. 470.)—Dr. Roget, V.P., in the chair.—Mr. Gray "On the Manufacture of Shell Cameos." The author commenced by stating that the ancients formed cameos by engraving figures in low relief on different kinds of siliceous stones; and generally selected for that purpose those which had layers of different colors, so that the figures were of divers colors. Such cameos are now made in Southern Europe and in France, where this art has lately been attempted to be revived: but the hardness of the materials requires so much labor that they are too expensive to come into general use. Numerous attempts have been made to substitute various materials, such as porcelain and glass, for the ancient cameos, but their great inferiority has caused them to be neglected. The best and now most used substitutes are shells; several kinds of which afford the necessary difference of color, and are, at the same time, soft enough to be worked with ease, and hard enough to resist wear. The shells used are those of the flesh-eating univalve, which are peculiar as being formed of three layers which are often of different colors, as they afford him the means of relieving his work; but the kinds now employed, and which experience has taught him are best for his purpose, are the Bull's Mouth, the Black Helmet, the Horned Helmet, and the Queen Conch. The first two are the best shells. After detailing the peculiarities of these shells, the writer proceeded to give an account of the progress of the art, which was confined to Rome for upwards of forty years, and to Italy until the last twenty years, at which period an Italian commenced the making of them in Paris; and now about three hundred persons are employed in this branch of trade in that city. The number of shells used annually, thirty years ago, was about three hundred, the whole of which were sent from England; the value of each shell in Rome being thirty shillings. To shew the increase of this trade, the number of shells used in France last year was nearly as follows:

Bull's Mouth,	80,000,	average price	1*s.* 8*d.*,	value	£6400
Black Helmet,	8,000,	"	5*s.* 0*d.*,	"	1800
Horned Helmet,	500,	"	2*s.* 6*d.*,	"	60
Queen Conch,	12,000,	"	1*s.* 2½*d.*,	"	700
	100,500 shells.			sterling,	£8960

The average value of the large cameos made in Paris is about six francs each, giving a sterling value of £32,000; and the value of the

small cameos is about £8000, giving a total value of the cameos produced in Paris for the last year of £40,000; while in England not more than six persons are employed in this trade.

5. *Esculent Funguses of England*, (Athenæum, No. 1037, p. 948.)—The English are not a fungus-eating nation,—and in the "good old times" this might have been as much a boast as the common one, that we never eat frogs. Though we do not eat frogs like our neighbors the French, we are rather celebrated for our love of another of the reptilian family, turtle. There is no reason why we should eschew frogs, and relish turtle. There is still less for our eating one or two of the numberless edible funguses which our island produces, and condemning all the rest as worse than useless, under the name of "toad-stools." It is not so on the continent of Europe; where very generally the various species of fungi are esteemed agreeable and important articles of diet. The great drawback on the use of these esculents in this country is, that some are poisonous, and few persons possess the skill to distinguish them, with the exception of one or two species, from those which are edible. In the markets at Rome, there is an "inspector of funguses," versed in botany, and whose duty it is to examine and report on all such plants exposed for sale. The safety with which these vegetables may be eaten, has led to a very large consumption in that city, where not less than 140,000 pounds weight, worth £4000 sterling, are annually consumed. This in a population of 156,000 souls. We cannot estimate the value of funguses in our own country for an article of diet as less than in Italy; nor believe that the supply would be in a less ratio. If this be correct, the value of the funguses which are allowed to spring up and die, wasted in Great Britain, would be about half a million sterling in each year.

6. *Dr. Jos. D. Hooker's Botanical Mission to India*, (Lond. Journal of Botany for December.)—"The increased and increasing patronage which the government of this country affords to science, is a subject of high satisfaction to all naturalists. This patronage is peculiarly evinced in the liberality with which the treasures contained in the British Museum, and those in the Royal Botanic Gardens at Kew, are rendered available to the public good. In connection with the latter establishment, we have to announce that one of the most enthusiastic votaries of Botany, whose name stands at the head of the present article, has just quitted this country to further its interests. Dr. Hooker, having brought his *Flora Antarctica* to a close, has been appointed by H. M. Government to investigate the vegetable productions of India, and especially of the Himalaya Mountains; and as a treaty is now in progress between the British powers in Hindostan and the Chinese, with reference to the boundaries of Thibet, it is possible that even the latter interesting region may be visited by Dr. Hooker in the course of his journey. [In view of this, an extremely interesting letter from the veteran Humboldt, directing Dr. Hooker's attention to many important points which will demand examination,—relating to general physics and geology as well as natural history,—is published in this article; but it is too long to be extracted.] * * * After spending about twelve months in this undertaking, Dr. Hooker's instructions are to return in 1849 to Calcutta, and thence proceed to Singapore and Borneo. At the latter

island, the valuable aid of His Excellency, Mr. Brooke, and the protection furnished by H. M. S. Meander, (commanded by the Hon. Capt. Keppel, to which ship Dr. Hooker will be attached as Supernumerary Medical Officer,) will enable Dr. Hooker to fulfill the designs of the enlightened First Lord of the Admiralty, Lord Auckland, who directs that he shall pursue his botanical researches, and draw up a report on the vegetable productions of the British settlement of Labuan, and such parts of Borneo as can safely be explored. It is especially his object to ascend, if possible, the great mountain of Kung Baloo, supposed to be 14,000 feet in height. Dr. Hooker embarked at Portsmouth, on the 11th of November, in H. M. Steam-frigate Sidon, which conveys Lord Dalhousie to Alexandria en route for Calcutta. * * * Two or three months will probably be devoted to investigating the plains of Bengal, and particularly the fossil vegetable remains in the coal-formations of Burdwan; and he will then journey northward, perhaps to Sikkim; but his exact route must considerably depend upon circumstances which it is impossible yet to foresee."

7. *Collecting of Peruvian Bark*, (Jameson's Jour., July, 1847, from Travels in Peru, by Dr. J. J. von Tschudi, p. 398.)—In the month of May, the Indians assemble to collect Peruvian bark, for which purpose they repair to the extensive Cinchona woods. One of the party climbs a high tree, to obtain, if possible, an uninterrupted view over the forest, and to spy out the *Manchas*, or spots where there are groups of Peruvian bark trees. The men who thus spy out the trees are called Cateadors or searchers. It requires great experience to single out of the dark leaf-covered expanse, the Cinchona groups, merely by the particular tint of the foliage, which often differs but very little from that of the surrounding trees. As soon as the Cateador has marked out and correctly fixed upon the mancha, he descends to his companions, and leads them with wonderful precision through the almost impenetrable forest to the group. A hut is immediately built, which serves as a resting-place during the night, and is also used for drying and preserving the bark. The tree is felled as near the root as possible, divided into pieces, each from three to four feet long, and with a short curved knife a longitudinal incision is made in the bark. After a few days, if the pieces are found to be getting dry, the bark already incised is stripped off in long slips, which are placed in the hut, or in hot weather, laid before it to dry. In many parts, particularly in the central and southern districts of Peru, where the moisture is not very great, the bark is dried in the forest, and the slips are packed in large bundles. In other districts, on the contrary, the bark is rolled up green, and sent to the neighboring villages, where it is dried. Towards the end of September, the *Cascarilleros** return to their houses.

In the more early periods of South American history, the bark was a principal article of Peruvian commerce. Since the commencement of the present century, its value has, however, considerably diminish-

* Bark-gatherers. The Peruvians call the bark cascarilla, and they point out the distinction of a great number of species and varieties.

ed, chiefly in consequence of adulterated and inferior kinds, which are supplied from other quarters; perhaps, also, on account of the more frequent use of quinine; for, in the production of the alcaloids less bark is employed than was formerly used in substance. During the war of Independence, the bark trade received its death-blow, and for the space of several years, scarcely more than a few hundred weights of bark were exported from Peru. The Montanas of Huanuco, which once furnished all the apothecaries of Europe with the "divine medicine," are beginning again to yield supplies. From the roots of the felled trees a vigorous after-growth has commenced. In the Montanas of Huamalies a kind of bark is found, the nature of which is not yet defined by botanists; and from the Montanas of Urubamba comes the highly esteemed *Cascarilla de Cuzco*, which contains an alcaloid, named *Cusconin*.* Possibly the medical bark may again become a flourishing branch of trade for Peru, though it can never again recover the importance which was attached to it a century ago. During my residence in Peru, a plan was in agitation for establishing a quinine manufactory at Huanuco. The plan, if well carried out, would certainly be attended with success. There is in Bolivia an establishment of this kind conducted by a Frenchman; but the quinine produced is very impure. The inhabitants of the Peruvian forests drink an infusion of the green bark, as a remedy against intermittent fever. I have found it in many cases much more efficacious than the dried kind, for less than half the usual dose, in a short time, produces convalescence, and the patient is secure against returning febrile attacks.

8. *Canaan (Ct.) Native Iron;* by STEPHEN READ, (in a letter to Professor Silliman, dated Richmond, Mass., Dec. 20th, 1847.)—You may recollect that in connection with our mutual friend, Professor Shepard, I was asked by the Association of Geologists and Naturalists, at their meeting in New York, to make some examination relative to the native iron, said to have been found in the town of Canaan in your state. I have spent some time in the examination, visiting the spot three or four times, but could find nothing. As the amount of business before the Association at their meeting in Boston was very great, I thought it not best to occupy their time with a mere negative report. But knowing the interest you had felt in the subject, I thought you would not deem it an intrusion should I drop you a line in relation to it. Some thirty years since, it became necessary to ascertain to which of the two societies in Canaan, belonged a certain family living on the remote (eastern) part of Canaan Mountain. Wm. M. Burrall, Esq., County Surveyor, and since Judge of one of your courts, was called to run the line. A conspicuous object on the west side, and at the foot of the mountain, was a known boundary. The company took their first station on the west ridge of the mountain, "defining their position" by the bearing of the old chimney, some 800 or 1,000 feet below them, and nearly a mile distant. On another ridge perhaps a hundred rods distant, an object having such peculiarities that it might be known when

* From Cuzco, the ancient residence of the Incas. It was discovered by the French chemists, Corriol and Pelletier, in the Cascarilla which is shipped in Africa; hence this alcaloid is also called *Aricin*.

reached, was found to be in the line of the survey. The north part of the intervening valley, is covered most of the year by a pond of water. The south edge of this is near the line. In the other part of the valley, rocks, alders, laurels, &c. vie for the supremacy. As no line was to be traced, distance measured, or stations preserved, the object of the survey being yet two or three miles distant, each of the parties passed this valley, where in his own judgment the fewest obstacles would be found. In doing this, Judge Burrall found a something which he supposed to be plumbago, (to which his attention had been called some short time previous,) projecting a little from a white flint (quartz) rock. After much pounding with a stone, he succeeded in detaching a piece, put it in his pocket, and hastened on. The specimen was carried home, and thrown into his desk. Sometime after this the attention of his son, Wm. P. Burrall, then in College, was called to this specimen, which he had often seen in his father's desk in the office, by hearing a lecture on native or meteoric iron, accompanied by specimens; and at the close of the next vacation, he carried the specimen to New Haven. Every thing subsequent to that is well known to yourself. I had recently finished my own collegiate course, and was at home in Canaan, and made one of the first company which went to the supposed locality to search for it. I have conversed often with Judge Burrall upon the subject, once since our meeting in Boston, and two or three times during the last year. He feels confident there can be no mistake about the specimen. And when we recollect that the specimen passed from his hands more than twenty years since, and compare his recent statement that he supposed it plumbago, with the fact that the specimen is coated with plumbago, that he broke it from a white flint rock, while fragments of quartz still adhere to it, that it had been so carefully noticed by the son, that he at once recognized its relationship to the specimens seen at New Haven, surely we must acknowledge that the moral evidence of its genuineness is exceedingly strong. The difficulties attending its rediscovery are not small. The time which had elapsed before any search was made, was sufficient to have covered it with brush, leaves, moss, &c. The nature of the ground, and the uncertainty as to the exact place where the valley was crossed, afford serious difficulties. I had hoped the magnetic needle might have aided, but the rock of that part of the hill (mica slate with veins of quartz) is so fully charged with magnetic oxyd of iron, as to cause a large variation in the needle. I had hoped to have been accompanied in my examination, by the Judge himself; but there are but few days in a year, when his health and strength, and other cares, would permit so severe an excursion; and living thirty miles from each other, we have not, and I fear shall not meet on one of those fortunate days.

9. *Fall of an Aerolite at Forest Hill, Arkansaw.*—A letter from Henry Hicks, P. M., dated Dec. 12, 1847, to the editor of the Philadelphia Courier, states, that on the 8th of December the sky was perfectly clear, and had been so through the day; but at quarter past 3 P. M., it became suddenly very dark. The clouds, or what appeared to be such, whirled in the strangest contortions, and appeared like a solid black fleece lighted from above by a red glare as of many torches, but scarcely penetrating the masses of clouds below. In the presence

of hundreds of spectators there was a deafening explosion, and the concussion shook the houses and caused the bell of the village church to toll several times. At the moment of the explosion an ignited body—compared for size to a hogshead—"descended like lightning," and struck the earth about twenty feet east of a cotton gin which stands near the village. Within twenty minutes, the sky was as clear and the sun shone as brightly as ever. The hole made by the aërolite was eight feet deep and two feet two and a half inches in diameter; at the bottom of it was a black mass, which was still so hot that water thrown upon it hissed as it would do on hot iron. The exhumed mass, recovered with considerable labor, was covered by a black incrustation; it smelled of sulphur, and was, for weight, compared to common rock. It is not stated that it was weighed. [We look for specimens confirming this event.]

10. *Original Columns of the Giants' Causeway, and Model of this wonderful group of Natural Pillars.*—The Cabinet of Yale College has received the above named valuable present, from one of her most respected and loyal alumni, Lucius C. Duncan, Esq., of New Orleans, of the graduating class of 1821.

Mr. Duncan being at the Causeway in August, 1847, obtained six joints of this magnificent colonnade—truly one of the wonders of the world—and caused them to be shipped for New York; they arrived in due time and are now in Yale College safe and uninjured. They have added interesting means of illustration upon this topic of geology, and are rendered the more effective by the beautiful model which came with them. It is constructed of wood, painted black—the columns being represented in their natural grouping.

Geological models are now extensively employed, to convey to the mind through the senses, a correct idea of form, proportion, and arrangement, especially of remarkable geological formations, and nothing can in that way be more effectual. Our institutions would be greatly benefitted if the friends of science, travelling among interesting scenes, would more frequently imitate the laudable example of Mr. Duncan.

11. *Academy of Natural Sciences of Philadelphia.*—The following facts gathered from recent Annual Reports to the Academy, will be read with pleasure by all interested in the progress of science in this country.

The year has been one of unusual prosperity and progress. This great success is mainly due to the munificence of Dr. Thomas B. Wilson, whose purchase and deposit with the Society of the Rivoli collection of birds, has already been announced in our pages. This collection contains 12,500 specimens. It is particularly rich in the Rapacious birds, but contains also extensive suites of species of the other orders. The pigeons number 120 species; the parrots 200; the species of Paradise birds are all present in the collection. But we must refer to the Report on the collections, for details. Dr. Wilson since his purchase of the Rivoli collection, has also procured Mr. John Gould's collection of Australian birds, which contains specimens of all the known species from that region excepting *five*, and the nests and eggs of a large number. The collection acquires additional interest, from its containing the original specimens from which many of the species instituted by Mr. Gould,

were figured and described for his splendid work on Australian Ornithology. The Academy's collection have also been enriched by Dr. Wilson with five nearly perfect skeletons of large fossil Saurians. One of these is the Plesiosaurus Hawkinsii, three are species of Ichthyosaurus, and the fifth is the Teleosaurus. Numerous valuable and costly additions to the library have been made by the same gentleman. And for the accommodation of these accessions to the Society's collections, Dr. Wilson has incurred a large expense in adding to the size of the building, remodeling the basement rooms and making them as well as other parts of the building fire-proof.

The Society has also received large donations in different departments, from Dr. Hallowell, Dr. T. S. Savage, Mr. Wm. H. Edwards, Dr. R. E. Griffith, Mr. R. E. Kilvington, Mr. Edward Harris, Mr. Wm. Gambel, Mr. Cassin and many others.

The additions by deposit and presentation are as follows:—To the

Library, . . .	1072 additions.
Mammalogy, . . .	91 specimens.
Ornithology, . . .	23,000 specimens.
Herpetology, . . .	88 species.
Conchology, . . .	1893 species and almost 2300 varieties.
Entomology, (besides many not included,)	1035 specimens.
Comparative anatomy, .	385 crania, besides several skeletons.
Palæontology—Large Saurians,	6 specimens.
Fossil shells, &c.,	over 12,000 specimens.

A notice of the recent number of the Journal of the Academy in a quarto form, is given on a following page under Bibliography.

12. *Lake Baikal*, (Jameson's Jour., July, 1847, from Narrative of a Journey round the World, by Sir GEORGE SIMPSON. Vol. ii, p. 381.)—Lake Baikal is about seven or eight hundred versts in length, and about seventy or eighty broad at its widest part. The waters are as clear as crystal, everywhere deep, and in many places unfathomable. Besides the numberless cascades that rush down its walls of mountains, it receives many rivers, more especially the Angara at its northern extremity, and the Selenga on its eastern side, towards the south; and its single outlet, in spite of the superior claims of the Selenga, on the double ground of position and magnitude, professes in its name to be a continuation of the remote and comparatively inconsiderable Angara. The two Angaras are sometimes distinguished from each other as Upper and Lower. The quantity of water which issues from the lake is believed to be vastly less than that which flows into it, the difference being, in all probability, too great to be explained by evaporation alone. In this view of the thing, a large portion, as a matter of course, must be absorbed, an operation which the volcanic origin of the huge hollow may be supposed likely to facilitate. In fact, the lake presents certain features which have induced individuals to infer that it has a subterranean communication with the ocean. It is the only body of fresh water in the world that possesses Phocæ or seals; and when agitated, in the way already mentioned, by invisible causes, it throws up to its surface quantities of small fish, which are never seen at any other time. In illustration of the mysterious agencies of nature, which pro-

duce the same ends by contrary means, I subjoin two passages from Baron Wrangell's interesting work.

"These flat valleys are occasionally filled with water, by the overflowing of the rivers in spring, when they form lakes of various sizes, all very full of fish. The intense frosts of winter cause large clefts in the ground, by which the water drains off, sometimes in the course of a single year, sometimes in several.

"A curious phenomenon occurs in the lakes in the vicinity of the village of Alaseya. In the middle of winter, the water sometimes suddenly disappears without any side channels being visible. In such cases a loud noise is heard at the time the water disappears, and, when the bottom of the lake is laid bare, large clefts are visible, occasioned by the severity of the frost."

13. *Chikiswalungo Iron Furnace, near Columbia, Pa.;* by S. S. HALDEMAN, (in a letter to one of the editors of this Journal.)—The Chikiswalungo furnace, is thirty-two feet high and eight feet greatest diameter; it is driven by a forty horse power engine. The bellows is sixty inches in diameter, its stroke five and a half feet, and its crank makes fourteen revolutions in a minute. It was built as a "forty ton" furnace, but owing to the constant attention and the theoretical knowledge of my brother and partner, Dr. E. Haldeman, the average product is sixty-five tons a week. The following table exhibits an unusual good week's work, although, if scrap iron had been used with the ore, the result would have been one or two tons more. Anthracite coal and a hot blast are used.

Sept., 1847.	Charges.	Coal.	Ore.	Limestone.
26,	37	900 = 33,300 lbs.	1400 = 51,800 lbs.	425 = 15,725 lbs.
27,	36	900 = 32,400 "	1400 = 50,400 "	425 = 15,300 "
28,	36	900 = 32,400 "	1500 = 54,000 "	435 = 15,660 "
29,	37	900 = 33,300 "	1500 = 55,500 "	435 = 16,095 "
30,	36	900 = 32,400 "	1500 = 54,000 "	435 = 15,660 "
Oct. 1,	36	900 = 32,400 "	1400 = 50,400 "	425 = 15,300 "
2,	36	900 = 32,400 "	1400 = 50,400 "	425 = 15,300 "
		228,600 "	366,500 "	109,040 "

228,600 lbs. = 102 T., 1 cwt., 0 qr., 08 lbs. 366,500 lbs. = 163 T., 12 cwt., 1 qr., 08 lbs. 109,040 lbs. = 48 T., 13 cwt., 2 qr., 08 lbs.
Result of the above, 72 tons pig metal.

To explain the above. On the 26th of September the furnace was filled thirty-seven times, each charge containing 900 lbs. of coal, (making 33,300 lbs. in the twenty-four hours,) 1400 of ore ("chestnut hill" hematite), and 425 of flux The engine consumes forty tons of coal a week, not taken into the above account; but we intend to make such alterations in the spring as will cut off this expense.

When a furnace is blown in, the hearth and stack being cold, the ten first days are counted as a week's work. Here follows the result of such a week (of but eight days, however,) ending with the 30th of October last.

	Tons.	Cwt.	Qr.	Lbs.
Coal, . . .	98	08	3	00
Ore, . . .	127	02	3	12
Flux, . . .	41	19	2	21

Result, 43 tons (gross weight) of pig metal.

14. *Telegraphic Reports of Meteorological Phenomena.*—Messrs. Jones & Co., Merchants' Exchange, New York, have made arrangements to give daily and hourly reports of meteorological phenomena, by telegraphic messages from all parts of the country which are in telegraphic communication with New York. This novel and important enterprise will furnish more extensive means of synchronous comparison of the state of the barometer, direction of the wind, and generally of all meteorological phenomena, than were ever before possessed by the scientific world. It is hoped the colleges, scientific institutions, and individuals favorably situated, will combine their efforts to give efficiency to this scheme, which if properly encouraged by proper hands, cannot fail of interesting results.

We refer to the advertisement of Messrs. Jones on our advertising sheet.

15. *Italian Congress of Science.*—At the late session, held at Venice, there were fifteen hundred men of science assembled from Italy and various parts of Europe, and a writer observes that nothing could exceed the magnificence of the Italian Congress. The meeting continued fifteen days; and at the close, ten members of the Geological Section, made an excursion to the Alps.

16. *Note on Nineveh;* by AZARIAH SMITH, (from a letter to B. Silliman, Jr., dated Sivas, Nov. 15, 1847.)—In my article on Nineveh, I think I said something (for I have nothing yet to refresh my memory with) to account for the burying up and preservation of the ancient tablets, &c. there described. A word to make this point more clear. The climate of that region is such, that during the summer months* all the wealthy seek for cool air in the middle of the day, in "serdabs" or rooms beneath the surface of the ground. The kings of former days, whose coffers were equal to the expense, seem to have secured the same object in a better way. By erecting a mound of sufficient height, they were able to have serdabs in a situation to command a free circulation of air and a fine prospect, and to secure at the same time, coolness of air and freedom from moisture. It is to the *thick* roof of earth required to exclude the heat of the rays of the sun, and which, when its support was burned, filled up to a great depth the rooms beneath, that we owe the preservation of those marbles from the ravages of time, and the destroying hand of those who have since inhabited that region.

VI. BIBLIOGRAPHY.

1. *A Manual of the Botany of the Northern United States, from New England to Wisconsin and South to Ohio and Pennsylvania, inclusive, (the Mosses and Liverworts, by* WM. S. SULLIVANT,) *arranged according to the Natural System; with an Introduction, containing a reduction of the genera to the Linnæan artificial classes and orders, outlines of the elements of Botany, a Glossary, etc.;* by ASA GRAY, M.D., Fisher Professor in Natural History in Harvard University.

* See an article on temperature of Western Asia—Mosul. This Journal, ii ser., ii, p. 72.

12mo, 800 pages. Boston and Cambridge: James Munroe and Company. London: John Chapman. 1848.—This work which we announced some time since is now ready, and as we have been favored with an early copy of the contents and preface, we cannot do better justice to the learned author than by copying a portion of the latter.

"This work is designed as a compendious Flora of the Northern portion of the United States, arranged according to the Natural System, for the use of students and of practical botanists. It was intended to be comprised within the compass of a pocket volume, which might serve as a *vade-mecum* in herborizations, as well as a convenient manual of reference at home. But the volume has attained a somewhat unwieldy bulk, notwithstanding every effort at condensation, and the rigorous exclusion of all irrelevant matter, however interesting in itself, and of all synonymy not really essential." * * * *

"The wide district which this compendious Flora embraces, although irregular in form, plainly belongs to one and the same botanical region. With the exception of the small patches of alpine vegetation which crown the higher mountains of Northern New England and Northern New York; of the sea-side plants, and of some appropriately Southern forms which not only reach Delaware and New Jersey (especially the Pine barrens), but also straggle northward coastwise, in diminishing numbers, quite to New Hampshire; of a very few which belong to the Great Lakes; and perhaps a larger number of Western prairie plants which extend into Ohio, Wisconsin, and Michigan:—with these exceptions, the vegetation is remarkably homogeneous for so large a district, and a very great proportion of the species are sporadic over the whole breadth." * * * *

"The more striking and distinctive points of the ordinal character are brought together and printed in italics in the first sentence of the description of each order, so that they may the more readily strike the student's attention. To abridge the labor of analysis as much as possible, I have given an easy synopsis of the genera under each order, whenever it comprises three or more of them; I have been particular to dispose the species of every extensive genus under sections or subgenera (§), subsections (*), and subordinate divisions (+, ++, &c.), founded on obvious characters; and, whenever there are two or more species under a division, I have italicized some of the leading distinctions (after the manner of Koch's *Flora Germanica*), so that they may at once catch the student's eye. The full-face type, in which the names of the genera and species are printed, affords a similar facility, by rendering them very conspicuous. To aid in their pronunciation, I have not only marked the accented syllable, but have followed Loudon's mode of indicating what is called the long sound of the vowel by the grave (ˋ), and the short sound by the acute accent mark (ˊ). In respect to this, my friend, Mr. Folsom, has obligingly rendered most important assistance."

"Finally in order to render this Manual complete and sufficient in itself for the study of our plants, I have prefixed a concise Introduction to Botany, both Structural and Systematical, which, with the annexed Glossary and Index combined, should serve to convey the requisite elementary knowledge of the science, and to explain all the technical

terms usually employed in botanical descriptions. Very many of these terms, however, are not used at all in the body of the work; for I have throughout endeavored to smooth the beginner's way by discarding many an unnecessary technical word or phrase, and by casting the language somewhat in a vernacular mould,—perhaps at some sacrifice of brevity, but not, I trust, of the precision for which botanical language is distinguished." * * * *

"An artificial analysis of the Natural Orders or Families, founded on the easier and chiefly external characters, is appended, to aid the learner at the point where he is likely most to need assistance, namely, in referring an unknown plant to its proper order. Every successful attempt, however, will give him new power; and the thorough study, by this system, of half a dozen plants of different families will give more knowledge and insight into the science than can be acquired by ascertaining the names (which is nearly all that is done) of a hundred species by means of a facile, thought-saving, artificial classification. Still, as some artificial analysis which leads directly to the genera is a great convenience to the student in cases of difficulty, I have added a full Conspectus of the genera comprised in this work, reduced to the Classes and Orders of the Linnæan Artificial System, so generally in use until recently. The plan upon which this is constructed will be found to possess some advantages over an ordinary Linnæan arrangement, inasmuch as it provides for the exceptional cases, where different species of a genus actually present the characters of different artificial classes."*

2. *Report of the Superintendent of the Coast Survey, showing the progress of the work during the year ending October*, 1847. (Senate Document, 30th Congress, 1st session, Executive, No. 6.) Our coast survey is making good progress. The following are a few paragraphs from this valuable Report.

"During the past year, field or office work has extended into the following *eighteen States on the Atlantic and Gulf of Mexico:* Maine, New Hampshire, Massachusetts, Rhode Island, Connecticut, New York, New Jersey, Pennsylvania, Delaware, Maryland, Virginia, North Carolina, South Carolina, Georgia, Alabama, Mississippi, Louisiana and Texas. The estimates for the next year will provide for work in every state on the coast of the Atlantic and Gulf of Mexico.

"As the work advances in each section, the results are collected and will be published, each separate centre of work furnishing the same means for preparing preliminary charts, and the survey being carried on upon the same general plan, and by the same general methods in each section. The whole will finally form a connected survey of the coast of the United States, and before that time each independent centre will yield the same scientific data for maps. A base line, data for geographical position, (latitude and longitude,) for distances between points; for configuration of the shore and of the adjacent country; for the depth of water, character of bottom, strength and direction of currents, and rise of tides, are furnished by the operations of the survey in each and every section, and are all sufficient for preliminary charts,

* This work, we understand, is sold at retail for $1,88, and to classes, schools, &c., at $1,50.—Eds.

much more exact than (with a few exceptions) those now existing. The connected whole will give the higher elements, which no detached surveys can furnish, necessary for the more elaborate computations and results.

"In the new sections, this plan enables us to give first attention to the points which, from their importance, or the want of knowledge in regard to them, most require it. As an illustration, may be mentioned the survey between Mobile and New Orleans, already in full progress, the results of which are important, not only locally and to the southwest, but to all the vast territory of the west which uses *New Orleans* and *Mobile* as its ports. This plan, too, reduces the estimate of the time of completion of the whole work to that of finishing each section, for which a few years afford approximate data, and the number of sections under survey, which must depend upon the appropriation deemed by the Executive and Congress consistent with an economical administration of the government. The gradual extension of such a work has the advantage of training up agents to the due performance of its different parts upon the same general plan, and of establishing a system in the details necessary to insure regularity and uniformity and effective control of the whole.

"Although the appropriate and most valuable results of the coast survey, are the furnishing of accurate maps and charts, yet there are striking results obtained from time to time which attest its value. In past years, the discoveries of Gedney's channel into New York harbor, of Blake's channel in Delaware Bay, of Blunt's, and of the breakwater, or through-channel, on the ridges of Cape May, of the new south shoal, near Nantucket, with minor discoveries, were of this class. The year just ended, has afforded its contributions of the same kind in the examination of the ridge near Nantucket New South Shoal, in the discoveries of a new shoal in the channel east of the 'Bass Rip,' near the island of Nantucket, and of a shoal southeast from Great Point light, near the same island, dangers before unknown, and to be carefully avoided by navigators, and in the discovery of a depth of water in the channel at the entrance of Mobile Bay of $20\frac{3}{4}$ feet. The first discoveries are important to the foreign, West India, and coasting trade; and the latter, not only to Mobile, but to the southwest generally. Particulars in regard to these discoveries are given in connexion with the progress of the work in a subsequent part of this report."

"The number of sheets of maps and charts now published, is sixteen; and, during the remainder of the fiscal year, at least five additional ones will be published. Besides these, two hydrographic sketches have been published, and two accompany this report."

The sections into which the coast of the United States is divided, are the following:

No. 1. From Passamaquoddy bay to Point Judith.
No. 2. From Point Judith to Cape Henlopen.
No. 3. From Cape Henlopen to Cape Henry.
No. 4. From Cape Henry to Cape Fear.
No. 5. From Cape Fear to the St. Mary's river.
No. 6. From the St. Mary's to St. Joseph's bay.
No. 7. From St. Joseph's bay to Mobile bay.
No. 8. From Mobile bay to Vermilion bay.
No. 9. From Vermilion bay to the boundary.

The charts as they are published, are struck off and afforded for sale at a very low rate. On page 52, it is stated that "Since the first of December, 1846, the following number of sheets have been printed: 484 copies of the small map of New York bay and harbor; 120 sheets of the large map; 1,481 copies of New Haven harbor; 645 of New Bedford harbor; 1,061 of Little Egg harbor; 380 of Annapolis harbor and the Severn river; 225 of the upper and middle sheet of Delaware bay and river; 240 of Fisher's Island sound; 1,471 of the harbors of Holmes's hole and Tarpaulin cove; 1,140 of Oyster bay or Syosset harbor; 400 of the sketch of the New South Shoal and Horn Island channel; making, in all, 7,647 sheets."

"Under authority of special act of Congress, and by direction of the Treasury Department, under the act reorganizing the coast survey, 1,104 maps have been distributed to scientific and literary institutions in the United States, (chiefly on application by members of the Senate and House of Representatives,) to departments of our own government, and to foreign governments, or used in the parties of the survey; 640 copies will probably be distributed in November. The distribution of the two sheets of Delaware bay and river has been sparingly made, on account of matching the sheets when the third is published—the different circumstances under which the sheets may be printed, or allowed to dry, determining a different shrinkage of the paper. 4,981 sets of maps have been turned over to the disbursing agent of the coast survey, to be placed with agents for sale. The low price at which the maps are afforded encourages the sale of them, and I have no doubt, that it ultimately will repay the cost of printing and paper."

3. *History of Vermont, Natural, Civil, and Statistical, in three parts, with a new map of the State and* 200 *engravings;* by ZADOCK THOMPSON. 8vo. Burlington, 1842.—Though six years have passed since the publication of this work, it merits special mention in this place. In addition to the ordinary information of a full gazetteer and history, it embraces a descriptive catalogue of the zoology of the state, a catalogue of its plants, and remarks on its geology and mineralogy. The zoology is illustrated with many wood-cuts, and much valuable information is given.

We cite from it the following altitudes of places in the state:—

The Chin, or north peak of Mansfield Mountains,	4279 feet.
Camel's Hump, ...	4183 "
Shrewsbury Mountain, ...	4086 "
The Nose, or south peak of Mansfield Mountains,	3983 "
Killington Peak, ...	3924 "
Equinox Mountain, Manchester, . . .	3706 "
Ascutney Mountain, ...	3320 "
Middlebury Turnpike, ...	2063 "
Montpelier Village, ...	485 "
Surface of Lake Champlain, ...	90 "

4. *Rapport sur la Séance Extraordinaire de la Société Impériale des Naturalistes de Moscou, du* 22 *Fev.*, 1847, a l'occasion du *Jubilé Semi-séculaire du Doctorat de S. Exc.* Mr. FISCHER DE WALDHEIM, fondateur de la Société; par le Second Secrêtaire, Dr. Renard. 72 pp. 8vo. Moscou, 1847.—The 22d of February last was made a day of

Jubilee at Moscow in honor of their illustrious philosopher, FISCHER DE WALDHEIM, the founder of the Imperial Society. The celebration was conducted under the auspices of his Imperial Majesty. The brilliancy of the occasion was highly honorable to the science of Russia, and a source of congratulation to Europe and not less to America. Gotthelf Fischer de Waldheim, was born at Waldheim (between Leipzig and Freyberg) in 1771. His investigations in science have extended through very many of its various departments. Among the letters from different scientific societies the following is here cited as "l'addresse de felicitation" from the Academy of Sciences at Munich:—

EXCELLENTISSIMO, DOCTISSIMO, EXPERIENTISSIMO DOMINO

GREG. J. FISCHER DE WALDHEIM

Philosophiæ et Medicinæ Doctori
Imp. Ross. status consiliario
multarum societatum literariarum Sodali
Ordinum amplissimorum plurium Equiti,
qui optime de historia naturali aliisque scientiis meruit
instituendo, gubernando, ornando
præclaram Societatem Cæsaream Naturæ Curiosorum Mosquensem,
ambiendo, arcessendo, acuendo
permultos historiæ naturalis fautores, patronos, amatores,
investigando, perscrutando, expiscando
remotas, ignotas, scopulosas Scythiæ provincias,
alendo, colendo, movendo
tam secundis quam adversis temporibus studiorum sacrum usum,
docendo, scribendo, edendo
anatomica, zoologica, palæontologica, geologica complura,
inquirendo, indagando, dilucidando
imprimis articulatorum Rossicorum genera, species, mores,
eruendo, exponendo, depingendo
recentium et pristinorum animalium ossa et reliquias,
viro summe venerabili,
qui patriæ ei germanæ et adoptivæ gloriam æque respexit,
et publice privatimque auxit
ACADEMIA SCIENTIARUM BOICA
veteris memoriam sodalitatis recolens
emensum singularem et memorabilem diem,
quo die ante hos quinquaginta annos doctoris gradum
est adeptus, piis cum votis gratulatur.
Monachii die 22 Febr. MDCCCXLVII.

5. *Principles of Physics and Meteorology;* by J. MULLER, Professor of Physics at the University of Freiburg. First American Edition, 636 pp. 8vo, with 538 engravings on wood, and two colored plates. Philadelphia: Lea & Blanchard, 1848.—Prof. Muller's Treatise on Physics is an excellent work, fully and elegantly illustrated. The extent and scope of the treatise will be gathered from the subjects of its sections: 1. General Properties of Bodies. 2. Equilibrium of Forces; in simple machines; between molecules; in liquids; between solids and liquids; in gases, &c. 3. Motion and Accelerating Forces. 4. Acoustics. 5. Light.

6. Magnetism and Electricity. 7. Heat. 8. Meteorology.—The work is popular in its mode of illustration, treating in a perspicuous style of the applications of physics, and much simplified by its numerous figures. It is printed in a superior manner and the impressions of the wood-cuts are peculiarly clear and unblemished, as will be observed in the specimen from the work annexed to the last number of this Journal.

6. *Daubeny on Active and Extinct Volcanos.*—Professor DAUBENY of Oxford has recently published a new and much enlarged edition of his Description of Active and Extinct Volcanos.

The present edition will be found to contain nearly twice the amount of matter included in the preceding one, embracing not only such new facts and observations with respect to volcanos as have been brought to light since its first appearance in 1826, but likewise the allied phenomena of Earthquakes and Thermal Springs as well as a fuller discussion of the theories connected with those subjects.

7. *Dr. Mantell's Wonders of Geology.*—This very interesting and instructive work has recently appeared in London in a new edition, thoroughly revised and posted up with great care to the present time by the learned author. We cannot doubt that the favorable opinion of the Wonders of Geology which we have heretofore expressed, will be fully sustained in the new edition.

An expected copy now on its way, will we trust enable us to give a fuller notice of this improved edition, in a future number.

8. *Journal of the Academy of Natural Sciences of Philadelphia*, New Series, vol. i, part I. pp. 1–94.—With this number, the Journal of the Academy appears in a large quarto form, handsomely printed, and furnished with highly finished illustrations. The octavo form, though more convenient for perusal, is quite inadequate for illustrations in many departments of science. The appearance of the volume is evidence of the prosperity of the Society, and the energy and science of its members. The following is a list of the articles.

I. On the Fossil Genus Basilosaurus, *Harlan*, (Zeuglodon, *Owen*,) with a notice of specimens from the Eocene Green Sand of South Carolina; by Robert W. Gibbes, M.D.: pp. 5 to 15, with five plates.

In this article, Dr. Gibbes gives a historical account of the discovery and notices of the Basilosaurus under its different names, describes the specimens examined by him, and recognizes three species.—These species are the *Basilosaurus cetoides*, Owen, *Basilosaurus squalodon* (the Squalodon of Grateloup, found a few leagues south of Bordeaux), and a third, the *B. serratus*, described formerly under the name of *Dorudon* by Dr. Gibbes.

The memoir is illustrated by five quarto lithographic plates.*

* In a Memoir on the *Zeuglodon* (Basilosaurus) *cetoides* collected from an examination of Koch's specimens, (and published in the Bulletin (Monatsbericht) of the Berlin Academy, June 1847,) *Müller* has distinguished two species, one with long vertebræ and the other with short, which he calls *Z. macrospondylus* and *Z. brachyspondylus*. He observes, that as the Basilosaurus of Harlan or the Zeuglodon cetoides of Owen was established on bones of both these species undistinguished, therefore their names cannot stand.

II. Notice of the discovery of a Cranium of the Zeuglodon (Basilosaurus); by M. Tuomey: pp. 16, 17.

III. Observations on Certain Fossil Bones from the Collection of the Academy of Natural Sciences of Philadelphia; by Richard Owen, Esq., F.R.S.: pp. 18–20.

IV. Description of a New Rapacious Bird (*Cymindis Wilsonii*) in the Museum of the Academy of Natural Sciences of Philadelphia; by John Cassin: pp. 21–24, with a colored plate.

V. Remarks on the Birds observed in Upper California, with descriptions of New Species; by Wm. Gambel: pp. 25–56, with two colored plates representing the *Parus montanus*, *Parus inornatus*, *Chamæa fasciata*, *Zonotruchus chlorura*, *Picus scalaris*, male and female.

VI. History and Anatomy of the Hemipterous Genus Belostoma; by Joseph Leidy: pp. 57–70, with a plate.

VII. Fragmenta Entomologica, (Species of the Genera Triplax, Lycus, Dyctioptera, Digrapha, Cænia, Celetes, Eros, Pedilus, Hybosorus, Canthon, Phanæus, Ochodæus, Bothynus, Zenoa, Monocerus, Cantharis, Pyrota, Heliophilus, Opatrum, Spondylis, Tetraopes;) by J. L. LeConte, M.D.

9. *Annals of the Lyceum of Natural History of New York*, Vol. iv, Nos. 10, 11, July 1847.—This number, from page 417 to 474, is occupied with a continuation of Dr. LeConte's Memoir on the Geodephagous Coleoptera. Next follows (pp. 475, 476) a description of a new species of Procellaria, (P. meridionalis,) by George N. Lawrence. This species was originally described under the name P. brevirostris, and as such is noticed in this Journal, ii Ser., vol. iii, page 436, which name was preoccupied by Lesson. The article is accompanied by a plate. The third and last article (p. 477 to 498) is on the distinctive characters of Cypræa reticulata of Martyn and C. histrio of Meuschen, by John H. Redfield; with a plate which will appear in the following number.

10. *Transactions of the American Philosophical Society, Philadelphia*, 4to, vol. x, New Series. Part I. Philadelphia, 1847.

I. Elias Loomis: Astronomical Observations made at Hudson (Ohio) Observatory. Third Series: pp. 1–15.

II. E. Nulty: Supplementary note on the constitution and different forms of the magic cyclovolute: pp. 17–25.

III. S. S. Haldeman: Materials towards a History of the Coleoptera Longicornia of the United States: pp. 17–66. [See page 133 of this volume.]

IV. Isaac Lea: Description of New Fresh Water and Land Shells of the United States: pp. 67–101, with 9 plates, [genera Unio, Anodonta, Margaritana, Melania, Anculosa, Schizostoma and Paludina.]

V. Wm. McIlvaine: Memoir explanatory of a New Perpetual Calendar, Civil and Ecclesiastical, Julian and Gregorian: pp. 103–129.

VI. Ch. D. Meigs, M.D.: On the Corpus Luteum: p. 131–139.

John C. Warren, M.D.: Etherization, with Surgical Remarks. 12mo, pp. 100. *Boston:* Ticknor & Co., 1848.

Matteucci: Lectures on the Physical Phenomena of Living Beings, translated by J. Pereira, M.D., F.R.S. 12mo, pp. 388. *Philadelphia:* Lea & Blanchard, 1848.

Smithsonian Contributions to Knowledge.—Occultations visible in the United States during the year 1848. pp. 11, 4to.

Capt. Sir James Ross, R. N.: A Voyage of Discovery and Research in the Southern and Antarctic Regions during the years 1839–1843. 2 vols. 8vo., with plates, maps and wood-cuts. *London*: John Murray, 1847.

J. Couch: Illustrations of Instinct, deduced from the habits of British animals. *London*: John Van Voorst, 1847.

David Milne, Esq.: On the Parallel Roads of Lochaber; with remarks on the change of Relative Levels of Sea and Land in Scotland, and the diluvial deposits of that country. 4to. *Edinburgh*: Neill & Co., 1847.

Journal of the Indian Archipelago and Eastern Asia. A monthly Journal, established at Singapore in July last. The numbers received by us are highly interesting, and will receive farther attention in a future number of this Journal.

Lorenz Oken: Elements of Physiophilosophy; translated from the German, by Alfred Tulk.

This philosophical volume is one of the publications of the Ray Society. It is by one of the most original thinkers of the age, to whom science is largely indebted for much of its recent progress; it will be read with interest for the singularity and profoundness of its views, although its philosophy is not admitted throughout to be correct.

M. Gaudin: Recherches sur le groupement des atomes dans les molecules et sur les causes les plus intimes des formes cristallines. broch, in 8vo. *Paris.*

Ch. Martins: Voyage Botanique le long des Côtes Septentrionales de la Norvège depuis Drontheim jusqu'au Cap Nord, (Extrait des Voyage au Scandinavie et Spitzberg de la Corvette *La Recherche.*) 8vo, pp. 138. *Paris.*

M. Sars: Fauna littoralis Norvegiæ, oder Beschreibung und Abbildungen neuer oder wenig bekannten Seethiere, nebst Beobachtungen über die Organisation, Lebensweise und Entwickelung derselben. 1st Heft, mit 10 Kupf. *Christiania*, 1846.

W. J. Broderip: Zoological Recreations. 1 vol. *London*: Henry Colburn. 10*s.* 6*d.* bound.

M. Perty: Ueber den Begriff des Thieres und die Eintheilung der thierischbelebten Wesen; mit tab. 8 broch. 10 Sgr. *Bern*: Huber & Co.

Dr. F. A. L. Thienemann: Die Fortpflanzungs-Geschichte der gesammten Vögel, nach dem gegenwartigen Standpunkte der Wissenschaft, mit Abbildung der bekannten Eier; mit 100 colorirten Tafeln. Zweites Heft. Bogen 7–12 und Tafeln xi–xx. Gr. 4. In Carton. Preis 4 Thlr. The 1st part appeared in 1845.

Dr. F. A. L. Thienemann: Rhea. Zeitschrift für die gesammte Ornithologie. Im Verein mit ornithologischen Freunden herausgegeben. Mit Abbildungen. Gr. 8. Jedes Heft 1 Thlr. 10 Ngr.

O. des Murs: Iconographie Ornithologique. *Paris*: Friedrich Klincksieck. Issued in Livraisons containing six plates of new figures dessined from nature, with text. The work is to be divided into series. The first series will contain about 60 Livraisons, a number of which have already appeared. Price per liv. of 4to edition, 8 francs; of folio edition, 12 francs.

Jac. Berzelius: Jahresbericht über die Fortschritte der Chemie und Mineralogie; 26th year; 1 and 2 parts.—Translated from the Swedish. *Tubingen*, 1847.

Proceedings of the Amer. Acad. of Arts and Sciences.—1847. *Aug.* 11. p. 165, Performance of the Cambridge Telescope; *W. C. Bond.*—*Oct.* 5. p. 168, Observations on Neptune and Mauvais's Comet; *W. C. Bond.*—p. 170, Series of Moon culminations; *W. C. Bond.*—p. 178, Possible number of Primary Planets; *Everett.*—*Nov.* 2. p. 183, Mitchell's Comet; *W. C. Bond.*—Satellite of Neptune; *W. C. Bond.*—*Dec.* 7. p. 185, Report on the Smithsonian Institution.—pp. 195–285, Synopsis of the Lichenes of the United States and British America; *E. Tuckerman.*—p. 285, Walker's Elements of Neptune.—p. 286, Formulæ for the perturbations of Neptune's longitude and radius vector; *Prof. Peirce.*—Elements of the orbit of the Satellite of Neptune; *Prof. Peirce.*—1848. *Jan.* 4. p. 296, Abstract of Le Verrier's first two memoirs, received from Le Verrier, and translated and submitted by Mr. Everett.

Proc. Acad. Nat. Sci. Philadelphia, vol. iii, No. 12. *Nov.* and *Dec.*, 1847. p. 313, On the slow destructibility of animal tissues in some cases; *J. Leidy.*—p. 315, Salamandra erythronota and S. cinerea of Green, distinct species; *S. S Haldeman.*—p. 317, Fossil fruits from the Nova Scotia coal beds; *R. Brown.*—p. 319, Horned viper of Western Africa (Cerastes nasicornis), with a plate; *E. Hallowell.*—p. 322, New fossil Ruminantia, with a plate; *J. Leidy.*—p. 330, A new species

of Buceros, and a notice of B. elatus; *J. Cassin.*—p. 332, Three new species of Icterus; *J. Cassin.*—p. 334–348, Reports on the progress and condition of the Society.—p. 348. The genus Heterodroma, Haldeman, identical with Psammœchus of Boudier; *S. S. Haldeman.*

Ann. & Mag. of Nat. History. London. No. 135. *Dec.*, 1847. On the structure of the fruit of the Cruciferæ; *L. C. Treviranus.*—Ornithology of Tobago; *W. Jardine.*—Two new mosses from Jamaica: *W. Wilson.*—New Cryptogamia from Jamaica; *T. Taylor.*—On C. J. Sundevall's paper on the birds of Calcutta; *E. Blyth.*—A new species of Hymenopterous insect; *F. Smith.*—Grasses and Sedges of Demerara; *R. Schomburgk.*—Note on Petasida ephippigera, an Australian grasshopper; *A. White.*—On the Indian Archipelago.—Zoological Society:—*G. Gulliver* on the Spermatozoa of the Indian Elephant; *P. H. Gosse* on the habits of Noctilio mastivus; *H. N. Turner, Jun.*, on the Distinction between the cervical and dorsal Vertebræ in Mammalia.

No. 136. *Dec.* Supp., Zool. Society:—*C. L. Bonaparte* on a new South American bat; *B. H. Hodgson* on a new genus of Suidæ, and a new species of Taxidea; *E. Fry* on the circulation of Crocodilus lucius; *J. E. Gray* on the Cetacea of the British Islands.

Annales des Sciences Naturelles.—*May*, 1847.—On an organ in the Rays resembling the electric; *Dr. C. Robin.*—Mode of escape of urea after a removal of the kidneys; *Bernard* and *Barreswill.*—On the inferior types of the group Annelida; *A. de Quatrefages.*—Flora of Columbia; *L. R. Tulasne.*—On the plants in flower in the "Ecole du Botanique du Jardin des Plantes" at Paris, Feb. 28, 1847; *C. Martins.*—On the Parasites on roots referred to the order Rhizantheæ; *W. Griffith.*

August. On the circulation in Mollusca: *Milne Edwards.*—On the mechanism and the phenomena attending the formation of the embryo in the edible Echinus (E. esculentus); *Derbès.*—On the Saxicavous Annelida; *A. de Quatrefages.*—Embryogeny of the Annelida; *A. de Quatrefages.*—On the structure of the supra-renal bodies in man and other Vertebrata; *A. Ecker.*—On the organization of Vermes; *E. Blanchard.*—On the Artocarpeæ; *A. Trécul.*

September. On the Vermes (cont.); *E. Blanchard.*—On the modifications which take place in the egg of the hen during incubation; *M. Sacc.*—On the Artocarpeæ (cont'd); *A. Trécul.*—Respiration and structure of the Orobancheæ and other plants destitute of green portions; *C. Lory.*—Fourth notice on the Cryptogamic plants of France; *J. B. H. J. Desmazières.*

Bulletin de la Soc. Imp. des Nat. de Moscow, No. 2, 1847. Siberian Coleoptera; *F. Gebler.*—Saurian remains from Simbirsk; *G. Fischer de Waldheim.*—Paleontology of Moscow; *C. Rouillier* and *A. Vossinsky.*—Caucasian and Transcaucasian Curculionidæ; *J. Hochhuth.*—Jura limestone of Ciechocinck; *L. Zeuschner.*

Monatsbericht (Monthly Bulletin) of the Roy. Pruss. Acad. of Science at Berlin. MARCH, 1847. On Diffraction; *Magnus.*—Theory of the Steam-engine; *Crelle.*—APRIL. On the "Hydrarchos" of Koch; *Müller.*—Products from heating cyanids and double cyanids; *H. Rose.*—On thermoelectric decomposition, and a new eudiometric process; *Poggendorff.*—Composition of the Uranotantalite and Columbite of the Ilmen mountains, Siberia; *H. Rose.*—MAY. On separating Zinc and Antimony; *H. Rose.*—Microscopic Oraganisms in Ashes that fell on the island of Barbadoes, May, 1812; *Ehrenberg.*—Tertiary fossils near Berlin.—JUNE. Separation of Nickel and Cobalt; *H. Rose.*—On the genus Zeuglodon: *Müller.*—H. Clausius on the amount of light which the Earth derives from the reflection of the Sun's light by the Atmosphere; *Magnus.*—JULY. New Ammonites; *v. Buch.*—New product from the decomposition of Urea; *H. Wiedemann.*—Remark on the Yttrotantalite of Ytterby, Sweden; *H. Rose.*—Examination of the ash-results of burnt organic bodies; *H. Rose.*—New Planet; *Encke.*—Determination of magnesia in analysis by means of phosphate of soda, and of phosphoric acid by means of magnesia; *H. Rose.*